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Africanist Ethnographer and  
Physical Anthropologist in  
Early Twentieth-Century  
Germany and Poland

by Jan Czekanowski

edited by Adam Jones

**JAN CZEKANOWSKI**

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Early Twentieth-Century Germany and Poland**

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**THE SERIES "HISTORY AND CULTURE"**

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**THIS VOLUME**

*presents nine papers from a conference concerning Jan Czekanowski (1882-1965), who made his name as the ethnographer in the expedition of Adolf Friedrich, Duke of Mecklenburg, to East Central Africa in 1907-8. In what are today Ruanda, western Uganda and eastern Democratic Republic of Congo he collected material artefacts and skulls, ethnographic and other information, as well as recording music and speech. After returning to Poland he published the results of his research (mainly in German) and, in the 1920s and 1930s, became a leading specialist in the physical anthropology of Central Europe.*

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**Key words:** African studies, ethnography, Great Lakes, language, material culture, museums, National Socialism, phonograph, physical anthropology, race, Ruanda, Uganda

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# Jan Czekanowski

Africanist Ethnographer and  
Physical Anthropologist in Germany  
and Poland in the Early Twentieth  
Century

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All photographs except that of the phonograph were taken and published by Jan Czekanowski. The map on the title page was made by Leander Seige.

## Introduction

*Adam Jones*

Jan Czekanowski's career reflects many of the complexities of African studies and of the discipline of anthropology in the early twentieth century. Outside Poland he is remembered primarily for the detailed research he conducted in Central Africa in 1907-1909 as a member of the Duke of Mecklenburg's famous expedition, covering what are now Rwanda, northwestern Tanzania, western Uganda and northeastern Democratic Republic of Congo. The results were published in altogether five massive volumes - two (1917 and 1924) devoted to ethnography, two to illustrative material (1911 and 1927) and one to physical anthropology (1922). These publications took the form of a report, rather than that of an ethnographic monograph, although many of Czekanowski's individual remarks fitted well with this genre.<sup>1</sup> As several contributions to this volume emphasise, the ethnographic research was generally of a sufficiently high quality to survive the passing of various academic fashions; hence Czekanowski's writings continued to serve as a major source for anthropologists writing after the Second World War and indeed even today. Strictly speaking, Czekanowski was not an exponent of stationary fieldwork, the method developed by his countryman Bronislaw Malinowski a few years later. Nor, however, did he follow the example of ethnologists such as Leo Frobenius, for whom expeditions tended to lead from one place to another in rapid succession.

However, Czekanowski's fame does not rest exclusively upon his work as a collector of ethnographic data. Equally important - and laid down in the instructions he received from Felix von Luschan, the Africa custodian of Berlin's Museum of Ethnography - was the collection of other things: ethnographic artefacts, historical traditions, linguistic data, recordings of music and speech, and skulls. For one person to combine all these activities was not unusual in Czekanowski's day; but by the early 1930s such an all-round approach was generally considered dilettantist. Africanists had by then begun to specialise in particular disciplines, and it was not until relatively recently that the interdisciplinary (or at least multidisciplinary) approaches have again begun to influence African studies in a significant way.

The papers published here were presented at a conference held in Leipzig in October 2001 at the University of Leipzig's Department of African Studies (Institut für Afrikanistik)

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<sup>1</sup> Cf. Jan Vansina, "The ethnographic account as a genre in Central Africa", *Paideuma* 33 (1987), 433-44.

and at the Museum of Ethnography (Museum für Völkerkunde), which at the same time put on an exhibition of photographs and material artefacts, "Zwischen Nil und Kongo: Auf den Spuren von Jan Czekanowski", partly based on one prepared at the University of Warsaw. Thanks to the support of the Robert Bosch Foundation, four participants from Poland were able to attend the conference, and two of them - Joanna Bar and Krystyna Muszynska - are represented here.<sup>2</sup>

The volume begins with two papers on Czekanowski as a collector of ethnographic artefacts on behalf of the museums of ethnography in Berlin and Leipzig. **Christine Stelzig**, who co-authored a more lengthy study published elsewhere, discusses the sometimes strained relationship that emerged between Czekanowski and his 'employers' - the Duke of Mecklenburg on the one hand and Felix von Luschan, director of the Africa and Oceania department of the Berlin Museum of Ethnography on the other. **Christine Seige** complements this with a review of the ethnographic objects from Rwanda, Uganda and the Congo Free State (or Belgian Congo, as it became in 1908) in the Leipzig Museum of Ethnography, discussing the difficulty of ascertaining exactly which of these may be attributed directly to Czekanowski rather than to other members of the expedition.

There follow two papers on other kinds of material collected by Czekanowski on this expedition: first, the language data, which, as **Gerald Heusing** shows, were mostly assembled in a somewhat unprofessional manner but nevertheless include some information that can be of considerable value to modern linguists, and secondly the 84 wax cylinders recording music (and occasionally speech) from Rwanda - the earliest such recordings from East Central Africa -, which are discussed by **Susanne Ziegler**.

The three Polish contributions to this volume deal mainly with the ways in which Czekanowski's research findings have been viewed by later European anthropologists. **Joanna Bar**, whose doctoral thesis was devoted to Czekanowski's career as an anthropologist and Africanist, compares Czekanowski's published work with that of his predecessors and later researchers, noting how researchers built upon the findings of others. She discusses his handling of the Batwa, Pygmies and Azande, as well as of the Rwandan state and its history, examining how Czekanowski's findings were taken up by scholars such as Schweinfurth, Evans-Pritchard and Murdock, contrasting this generally positive reception with the relative neglect that Czekanowski experienced among his own countrymen. **Lidia Meschy** demonstrates that since the 1950s Belgian and French anthropologists and historians have made considerable use of Czekanowski's work on the interlacustrine region and that he is even mentioned on the official website of the Burundi government. **Krystyna Muszynska** likewise confines her focus to publications in one language, in this case English, and shows

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<sup>2</sup> The papers given by Leszek Dziegiel and Jadwiga Pstrusinska (both from Krakow) dealt with Africanist research after the period covered in this collection and hence have not been included here.

how various writers (notably Evans-Pritchard, Louis, Codere, Newbury, Iliffe and Pottier) have turned again and again to Czekanowski for information on such topics as the Zande, Rwandan demography and social structure, clientship and poverty.

Finally we have two papers which enable us to see Czekanowski in a more general context. **Sara Pugach** hardly deals with Czekanowski himself at all, preferring instead to discuss two of his contemporaries who, in their interaction with one another, encapsulated some of the dilemmas he too must have faced: Felix von Luschan, the museum custodian who selected Czekanowski to be the expedition's specialist in ethnography and physical anthropology, and Carl Meinhof, one of the founding figures of African linguistics in Germany. Just as Czekanowski tried to cover more than one discipline at the same time, so these two men sought to reconcile their different findings relating to the 'Hamites', the Pygmies and so on. The underlying assumption that language, 'race' and culture could somehow be classified together was a highly dangerous product of this drive towards interdisciplinarity, and fortunately did not significantly affect Czekanowski's own fieldwork.

After the First World War Czekanowski, now living in Poland, shifted his attention mainly to physical anthropology and in particular to that of Central Europe. **Katja Geisenhainer and Udo Mischek** examine critically the debates that took place in the 1930s both between and among German and Polish anthropologists regarding how 'Nordic' the Poles and their neighbours were, paying particular attention to the political context in which these debates took place.

We are left with an impression of a man whose enormous potential as a scholar was only partially realised, due partly to the circumstances of his career and the age in which he lived. His outstanding contribution to the ethnography of East Central Africa remains a valuable legacy today; yet from today's perspective it is curious that someone who was so capable of seeing beyond the stereotypes of his contemporaries in Africa should have devoted much of his later career to the study of race. Be that as it may, Czekanowski's work on Africa - as an author, photographer and collector - deserves to be studied far more deeply than has been possible so far. It is to be hoped that the meeting in Leipzig may prove to be the beginning of international cooperation in this field.





## **Czekanowski and the Museum of Ethnography in Berlin: A Tense Affair**

*Christine Stelzig*

In academic discussions concerning the collecting of the artefacts of non-European peoples in the late nineteenth and early twentieth centuries little attention has hitherto been paid to the complex intertwinements which often developed in Europe even before the collecting began. Political, financial and above all personal considerations could have a profound impact both on work "overseas" and on the aftermath "at home". I have already shown this in an article on Jan Czekanowski's relationship with the Berlin Museum of Ethnography, for which he worked as a practical trainee from 1907 to 1910.<sup>3</sup>

Czekanowski came to the Berlin Museum of Ethnography as assistant to Felix von Luschan (1854–1924), at that time director of the Africa and Oceania department, in 1907. He probably first caught Luschan's attention as a result of an article he wrote for the *Archiv für Anthropologie* in 1904. Czekanowski had intended to present the article at a congress of German anthropologists in Worms in 1903, but submitted it too late. Luschan was apparently so impressed by Czekanowski's knowledge that he immediately offered him an academic post and referred to the possibility that he might be able to join a scientific expedition to Central Africa – thereby excluding other possible candidates for the position of ethnographer. Czekanowski accepted the offer to join the Museum, but indicated that he first intended to finish his studies in Zurich. Felix von Luschan had hoped that by sending Czekanowski on the expedition, the Berlin Museum of Ethnography would acquire ethnographica mainly from the Uele-Ituri Region. Like most museum ethnologists at the turn of the century, in line with social Darwinist evolutionary theory, Luschan was convinced that the physical and cultural decline of the so-called *Naturvölker* was at hand as a result of the European impact, and was thus striving to acquire as much evidence of those cultures "facing extinction" as possible.

He had a clear idea of the scientific aims that his young assistant should pursue. At the top of his list was the systematic acquisition of ethnographic and anthropological objects. Czekanowski was instructed to use the Museum's own publication, *Anleitung zu ethnographischen Beobachtungen*, as a guide. He was to label the individual objects carefully,

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<sup>3</sup> This article is based upon a longer one by myself and Kathrin Adler, entitled "On the preconditions, circumstances and consequences of collecting. Jan Czekanowski and the Duke of Mecklenburg's expedition to Central Africa, 1907–8", *Journal of the History of Collections* 12, 2 (2000), 161–76. Unless otherwise indicated, the sources used are given there.

noting their indigenous name and, more particularly, documenting as accurately as possible how they were made. Furthermore, Luschan instructed Czekanowski to ensure that he acquired duplicates of everything collected, so that Berlin – in case of a dispute with the Museum of Ethnography in Leipzig concerning the division of collections between the two institutions – would not come off worst. In addition Czekanowski was expected to try to secure other objects for Berlin, inasmuch as he gained knowledge of them during the expedition, or at least to document them appropriately.

Czekanowski was under strict instructions to devote himself exclusively to ethnographic and anthropological work during the expedition and to place his services at the disposal of other areas of research only when extreme circumstances rendered his own work impossible. One of his explicit duties was to send a report to Berlin with every mail, as well as a complete carbon copy of his notes and the photographs taken. The Museum suggested that a normal day would produce three pages of diary notes. In addition to this Czekanowski was instructed to safeguard the Museum's interests "energetically and according to his utmost ability and the dictates of his conscience" and to try to interest as many people as possible in the ethnographic and anthropological work of the Museum.

The relationship between Luschan and Czekanowski can best be characterised as that of teacher and pupil or – considering the difference in age of almost 30 years – of father and son. In many of the letters to Luschan preserved in the archive of the Berlin Museum, Czekanowski addresses him as "highly esteemed teacher", refers to himself as his "most devoted pupil" and thanks him for his "great kindness and care". It is made clear repeatedly that Czekanowski set great store by Luschan's opinion and wanted to work only under his instruction.

However, despite this close relationship Luschan did not automatically support Czekanowski, as the discussion regarding how long the ethnographer would remain in Africa indicates. Not long after the beginning of the expedition Czekanowski was complaining about the Duke's haste, which was hampering him in his studies, and expressing regret that he was often not able to remain longer in one place. For this reason he hatched a plan as early as June 1907 to stay on in Africa for a further three months in order to do more research. He asked Luschan several times whether he might not influence the Duke to allow him to remain longer in one place. Luschan, however, urgently advised Czekanowski to remain with the expedition, for he feared a deterioration in the relationship with the Duke. In addition Czekanowski's idea of delaying his return to Europe by a year found little support with Luschan.

Luschan thus abandoned an idea he himself had expressed prior to the expedition. He now predicted problems with the Duke, who, he realised, could have no interest in postponing the publication of the expedition's findings. For this reason Luschan urged Czekanowski in



November 1908 once more to return soon, adducing reasons of health as well as financial factors. By all appearances only the lack of funding could move Czekanowski to return, and as late as February 1909 he informed Luschan about his concrete plans for a further journey to Africa, which would be exclusively devoted to physical anthropology.

Contrary to the agreement to publish the scientific results of the expedition within three years of his return, Czekanowski took 18 years to do so – a circumstance that generated widespread displeasure, the consequences of which dragged on for years and forced Luschan to intercede on Czekanowski's behalf on several occasions. Czekanowski appears to have fallen behind in his work by the end of 1910 and to have envisaged an extension of the date of submission; he asked the Museum's administration in December 1910 to be allowed to work on the expedition's material during the summer months of the years 1911–1914. Luschan supported his request, noting that Czekanowski had been given the “exclusive” right to publish his results within a period of three years after the expedition was concluded. This period would come to an end in the summer of 1912, but he, Luschan, did not have any reservations about extending the date of submission until the autumn of 1914. In this regard, Luschan was aware of the possibility that the Duke or the *Reichskolonialamt* might insist on publication of the results before 1914 or have other authors work on the missing chapters – and so it happened. At the end of 1910 Luschan submitted a request to Alexander von Danckelmann, a member of the *Reichskolonialamt*, which had been a substantial contributor to the expedition, for an extension of the submission date for Czekanowski's work. Von Danckelmann's reply was negative:

I have the great honour to inform the General Administration of the Royal Museums that His Highness the Duke Adolf Friedrich zu Mecklenburg, along with the Imperial Colonial Office and not least the publishers Klinkhardt & Biermann in Leipzig, earnestly desires that Dr. Czekanowski brings to a conclusion his work on the material from the expedition within the time agreed, as this would allow the whole work relating to the expedition to appear within the appointed time. I have unfortunately remarked that the number of cases in which members of scientific expeditions seek through unjustifiable means to withdraw from obligations and agreements freely entered into before the start of the expedition concerned, after they have attained their own immediate goals – without regard to the serious damage occasioned not only from a scientific but also from an economic point of view by procrastination of this sort – have recently risen markedly.

In my opinion all these agreements threaten to degenerate into a mere farce, if the royal authorities fail to put a stop to attempts of this kind by younger and rising academics to withdraw from obligations entered into and do not refrain from the now accustomed forbearance often shown them to date.

I make no secret of the fact that I lack any direct means of compelling Dr. Czekanowski to comply with obligations previously entered into, but I am of the opinion that a superior authority of the rank of the General Administration of the Royal Museums could well find ways and means to reduce, by means of moral suasion, the more egregious breaches of duty to a more bearable level.<sup>4</sup>

Luschan acted on Czekanowski's behalf with the Museum head administration by noting his industriousness and citing the move abroad as an excuse for the delay. In addition, he said, Czekanowski was "in a condition of greatly increased excitability" and regarded his (Luschan's) handling of this matter as a great injustice.

Whereas Czekanowski had a great advocate in Luschan, the succeeding directors of the Africa department were reticent. Moreover, ill-humour prevailed because Czekanowski only reluctantly returned copies of his notes to the Museum, which he had borrowed although he himself possessed the originals: At the beginning of January 1911 Czekanowski had signed a receipt for six volumes of files containing the carbon copies of his diary notes and the card index of the objects collected by him. He had borrowed these from the Museum in order to work on their publication, claiming that his original notes were incomplete. It would be many years before the borrowed material was returned to the Museum. An initial request for the return of the material was sent in December 1920 by Bernhard Ankermann, head of the Museum's Africa department since 1911. According to a letter sent from Warsaw in June 1921, Czekanowski sent Volumes I, III and IV of the diary notes to the Museum through the German legation, saying he had intended to bring the diaries with him to Berlin the previous year, but that the "Bilschwiki invasion"<sup>5</sup> had prevented this. Renewed and increasingly sharply worded warning notices from the Museum were sent again in December 1926 and in March and May 1928. The matter lasted on until May 1929, when the Museum noted the return of the documents – albeit incomplete.<sup>6</sup>

Around 1910/1911 Czekanowski's good relationship with Luschan began to deteriorate as well. Luschan – impatient at the sluggish pace of work – gave some of the linguistic material to a Berlin colleague without informing Czekanowski first, which resulted

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<sup>4</sup> Letter from Danckelmann to Luschan of 29 December 1909, SMB-PK, Ethnologisches Museum, I/MV 788: 53–54.

<sup>5</sup> Czekanowski was referring to the border war between Poland and the Soviet Union of 1919–1920, which ended with the peace treaty of Riga in March 1921.

<sup>6</sup> Letters from Czekanowski to the Museum of 10 December 1910, 20 June 1921, 21 May 1928, 13 December 1928 and 7 May 1929; Bernhard Ankermann to Czekanowski, 17 December 1920, and letters of the Museum's administration to Czekanowski, 20 December 1926, 12 May 1928 and 30 November 1928. SMB-PK, Ethnologisches Museum, I/MV 788: 51, 59–66.

in the latter's irrevocable anger.<sup>7</sup> After his retirement as director of the Africa and Oceania departments at the end of 1910, von Luschan continued to look after the Museum's anthropological collection until his death in 1924; yet although Czekanowski himself was completely engrossed in physical anthropology by this time, no further contact or correspondence appears to have taken place between the two scholars.

The reasons for the delay in publication are manifold and can only be guessed at in part. For one thing, after his return from Africa to Berlin in the middle of June 1909, Czekanowski's health was poor, and he was able to take up his duties at the Museum only in August. Later his move abroad – in 1910 he was appointed assistant to the ethnographic department of the Russian Academy of Sciences in St. Petersburg – his appointment as professor in 1913 and the outbreak of World War I all played a role in slowing down his work on the African material. Even more important, Czekanowski was the *only* scientist to work on and publish material from the expedition himself, in contrast to those who had collected botanical, geological or zoological material, for example: at one time as many as 45 scientists were engaged in working on the collected zoologica alone.

It is possible that yet another factor played a decisive role: the denigration of Czekanowski on the grounds of his Polish origin. This was unleashed as a result of a newspaper article in the Polish newspaper, *Slowo Polskie*, in which Czekanowski – according to a translation commissioned by Luschan – was reported to have told a fellow countryman that the collections put together by the Duke's expedition were due solely to him. The newspaper *Rheinisch-Westfälische Zeitung* used this remark to criticise the appointment of a Polish scientist as member of a German expedition. Under the headline “Polish Companion to the Duke”, it made a sharp personal attack on Czekanowski, referring to the *Slowo Polskie* as one of the “most anti-German newspapers in Galicia”. Supposedly, in an interview Czekanowski had not only admitted to being a Polish nationalist, but also maintained that he had collected all the ethnographic material that was later exhibited in Berlin as belonging to the Duke. Furthermore, in answer to the question whether or not he regretted that the objects remained in Berlin, Czekanowski allegedly replied with “nationalistic outrage” that in the future he wished to organize “expressly Polish research expeditions” as well. The author of the newspaper article closed with the remark:

The German public should be interested in clearing up this matter. For if it is true, as one must assume from this version, that the Museum's administration recommended a Polish person to the German Duke as a companion on the expedition, then one must question whether there really was no capable German person at hand who would have been equally deserving of such an opportunity. And if that was indeed not the case, should the choice have

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<sup>7</sup> Personal communication from Anna Czekanowska, 7 October 1995.

fallen upon a Russian Pole, who – as it has now become clear – was inimical to Germany? It is the old story once more: willing and with Slav cunning, adapting themselves to the circumstances, Poles take advantage of everything that German culture and science – together with German innocence – offers. Once they have it, however, they use it without reserve or tact to the greater glory of their own nation and against the Germans.<sup>8</sup>

The newspaper's claim that Czekanowski maintained he had collected “all of the ethnographic material” during the expedition that was “exhibited later as belonging to the Duke” was incorrect, as a note by Luschan dated June 1909 makes clear. Luschan took the issue very seriously, as several handwritten notes reveal, and he defended Czekanowski in general and in particular the directors of the Royal Museums of Berlin.<sup>9</sup> Czekanowski apparently felt himself compelled – although he may have been pushed into this by Luschan – to submit a statement of honour to the head administration, in which he stressed the contribution of his colleagues in assembling the collection jointly and emphasised that he regarded it as a great honour “to have been proposed by a Prussian official department for the journey”. Quite possibly this openly conducted debate was one of the reasons that prompted Czekanowski to leave Germany – and to revise, correct and publish the scientific findings of his journey as accurately and comprehensively as possible, even if this undertaking would take years.

A young anthropologist and ethnologist, freshly graduated with a doctorate, Jan Czekanowski was sent on a field expedition by the largest and most renowned ethnographic institutions in the German-speaking sphere at that time. At approximately the same time another colleague of the Africa department, Bernhard Ankermann, undertook a trip to Cameroun. Both expeditions can be considered pioneering efforts, for in both cases museum ethnologists travelled to Africa specifically to acquire ethnographica from and information about Africans. Both undertakings signified a new stage in the history of ethnology, in which data were increasingly collected by ethnologists themselves, rather than being derived from the reports of travellers. Although Czekanowski's journey cannot be regarded as fieldwork in the modern sense, it foreshadowed certain aspects of it. The fact that he stayed for a long time at certain stations and acquired some knowledge of several African languages reflects this.

The hopes and expectations that Luschan set in his young protégé were realised only to an extent. While Czekanowski's anthropometric measurements and the comprehensive collection of 1017 skulls were immensely significant for Luschan, he was nevertheless often dissatisfied with Czekanowski's approach to the collection of ethnographica. The latter frequently complained about the meagre presence of collectable ethnographica, the high

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<sup>8</sup> Rheinisch-Westfälische Zeitung, 9 June 1909.

<sup>9</sup> See SMB-PK, Ethnologisches Museum, I/MV 786: 213.

prices and the local population's refusal to part with its possessions, whereas he wrote with enthusiasm about the anthropometric measurements. All of this led Luschan to suspect that Czekanowski's attitude to the Museum's requirements was too one-sided and that he fulfilled his instructions with regard to artefacts at best only half-heartedly.



## **The Czekanowski Collection in the Museum of Ethnography in Leipzig<sup>10</sup>**

*Christine Seige*

At present there exists no comprehensive scientific treatment of the ethnographic collections of the German expedition to Central Africa 1907–1908 in the Museum of Ethnography in Berlin and in the Museum of Ethnography in Leipzig; nor has the history of the collecting of the objects and of the manner in which they were apportioned between the museums been studied in detail.<sup>11</sup> One difficulty in studying this history is the fact that many of the archival materials of the Berlin museum were lost during the Second World War; furthermore, at present the archives of the Leipzig museum are not accessible. Moreover, when one compares the inventories with the publications about the expedition uncertainties arise as to *which* of the collectors – Dr. Jan Czekanowski, the Duke of Mecklenburg or Lieutenant Walter von Wiese und Kaiserswaldau – actually collected which objects. The ethnographic collections consist of 4,000 objects,<sup>12</sup> which were in 1909 divided between the Museum of Ethnography in Berlin and the Museum of Ethnography in Leipzig. Above all the Leipzig museum owes this to Prof. Hans Meyer, joint owner of Leipzig's Bibliographical Institute and at that time chairman of the commission of the Reichskolonialamt, the government institution responsible for the exploration of the German colonies. Meyer had made an important financial contribution to the expedition with the aim of securing for the Leipzig museum a share in the collections. 370 objects collected by Czekanowski were brought to Leipzig, where - apart from some losses in the Second World War - they remain today.

Czekanowski is entered as collector with his own groups of object numbers in File 1909/3, "Collection of A. F. [Duke] of Mecklenburg", which contains 1400 objects. These are the numbers MAf ("Middle Africa") 17 863 – 18 248 and 19 922 – 19 933, as well as some doubles, registered in the Museum's book of incoming objects, vol. I. The same source also names Lieutenant Von Wiese und Kaiserswaldau as the collector. On the fiches Czekanowski and Wiese are registered as the collectors, but the head of the expedition, A. F. zu

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<sup>10</sup> This article is mainly based on one already published in German: "Die Sammlung Jan Czekanowski im Museum für Völkerkunde zu Leipzig", in the catalogue to the exhibition "Zwischen Nil und Kongo. Auf den Spuren von Jan Czekanowski" (Museum für Völkerkunde zu Leipzig, 2001), pp. 21-27.

<sup>11</sup> See, however, Stelzig and Adler 2000.

<sup>12</sup> Stelzig and Adler 2000, p. 167

Mecklenburg, as the former owner of the objects. The last-named is also often self-registered as collector. The collection of File 1909/3 was forwarded to Leipzig from the Museum of Ethnography in Berlin, where the head of the Africa Department, Prof. Felix von Luschan, had played a decisive role in making the expedition possible. Collections coming from Africa were registered first in the Museum of Ethnography in Berlin. The confusing nature of the data given in the file reflects the complicated and sometimes strained relationship between the organizers of the expedition, particularly with regard to its financing and to what constituted a corresponding share of the ethnographic collections.

Czekanowski took part in the expedition as an employee of the Museum of Ethnography in Berlin and was instructed to acquire ethnographic collections from the peoples he visited in what are now Rwanda, Uganda and north-eastern Democratic Republic of Congo. Much of his time, however, was taken up by the collection of information on physical anthropology. Since he also devoted himself to the study of the social organisation and political structures - which required time-consuming interviews and investigations - the collecting of objects and of data concerning their production and utilization was an important but by no means the only component of his research. Lieutenant Von Wiese und Kaiserswaldau too acquired numerous objects in Rwanda and in the Aruwimi region. He did this with the knowledge and consent of Czekanowski or - if the occasion arose - independently, with the permission of the Duke of Mecklenburg. For instance, he purchased in Niansa a fine collection when the expedition visited Musinga, King of Rwanda, at a time when Czekanowski was still residing at the mission of the White Fathers near Issawi.<sup>13</sup> The scientific members of the expedition made their tours separately in accordance with their specific research projects. Whereas the objects collected in the regions of the interlacustrine area in the first year of the expedition up to the disbandment of the expedition in Irumu west of Lake Albert in August 1908 were acquired by Czekanowski, Von Wiese and Mecklenburg, the objects from the northeastern Kongo, so from the Azande, Momvu or from the peoples from the Upper Nil region were collected by Czekanowski alone. Lieutenant Von Wiese, the Duke of Mecklenburg and the other European members of the expedition moved westward across the tropical forest to the Aruwimi, from where they travelled by ship to the Congo River and on this river to Kinshasa.

The Czekanowski collection of the Leipzig Museum for the interlacustrine area contains 238 objects from the Batoro, who live in the north of the Ruwenzori mountains in western Uganda. No objects from any other ethnic group are registered under his name; but probably many objects registered under the Duke of Mecklenburg's name were in fact collected by Czekanowski. This applies in particular to many of the objects from Kissaka in southeastern

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<sup>13</sup> Mecklenburg 1909, p. 125. On the history of Rwanda see Czekanowski 1917, chapter 14, and Ntezimana 1990, p. 73ff.

Rwanda, where Czekanowski travelled in July 1907 and from Mulera in northwestern Rwanda, where he stayed in September and October 1907<sup>14</sup> and studied, among other things, the agriculture of the Balera. From both regions Czekanowski published many photographs,<sup>15</sup> and his diary includes a list of the objects he purchased at the mission in Ruasa, Mulera.<sup>16</sup> The Duke of Mecklenburg, on the other hand, spent only one day at this place on his way from Gisenyi to the Virunga volcanoes.<sup>17</sup> Besides sorting, registering, packing and sending off the collections - as mentioned by the Duke - Czekanowski almost certainly collected the greater part of the objects acquired in Rwanda. This may be concluded from the chapters about material culture in Volume 3 of the *Forschungen im Nil-Kongo-Zwischengebiet*.<sup>18</sup> The lengthy observations regarding the production, use and purpose of the items collected and their role in the social life of the peoples strongly imply that it was he who purchased them. Indeed, in describing the wooden objects from Mulera which are today in the Leipzig museum, he says explicitly that he acquired them;<sup>19</sup> yet in the file they are registered – like all other objects from Mulera – under Adolf Friedrich, Duke of Mecklenburg, as collector. In the inventory books of the Museum of Ethnography in Berlin we find an even more clear example of Czekanowski's acquisitions being described as collected by the Duke: 46 objects from the Batoro are registered with the Duke as collector, whereas he himself wrote that he had not travelled through the Batoro region. It must be left to future research to throw light on this.

Numerically the objects of the Batoro constitute the largest group in Czekanowski's collection. In January 1908 Czekanowski stayed just under three weeks in Toro, the capital of the Toro kingdom, renamed Fort Portal by the English. He visited Dawdi Chwa Kassagama, the King of Toro, who gave him a manuscript about the history of his country and let him be present at a judicial hearing.<sup>20</sup> Until the 19th century Toro was part of the Bunyoro kingdom, one of the oldest states in the interlacustrine area, which until the 18th century had held hegemony over many ethnic groups of this region. In the 19th century Toro seceded as a result of conflicts within the ruling dynasty of Bunyoro. For a while, however, Kassagama and his predecessor could retain political independence only through the intervention of the

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<sup>14</sup> See Stelzig 2001, pp. 4f.

<sup>15</sup> Czekanowski 1911, plates 3-17, 21-40.

<sup>16</sup> Personal communication from Christine Stelzig, 21 August 2001.

<sup>17</sup> TB IB 70a Vol. 2: 294, 301, cited in Stelzig, 2001, p. 19, n. 85; Mecklenburg 1909, p. 207.

<sup>18</sup> Cf. Czekanowski 1917, chapters 7–9.

<sup>19</sup> Czekanowski 1917, p. 211.

<sup>20</sup> Cf. Czekanowski 1917, p. 55; 1958, p. 178.

British protectorate government in the internal conflicts of Bunyoro during the occupation of the northern territories of the interlacustrine area.<sup>21</sup>

In his late work *W głąb lasów Aruwimi. Dziennik wyprawy do Afryki Środkowej* ("Deep in the Forests of the Aruwimi. Diary of an Expedition to Central Africa", 1958) Czekanowski describes his working conditions in Toro as very good, despite the fact that he was ill for part of this time.<sup>22</sup> His collection of Toro objects contains many household articles, such as woven dishes and baskets, many different calabashes made into ladles, drinking cups and beer receptacles, a series of black earthenware vessels and bowls with a spherical base for milk, water, beer and cooking, as well as various kinds of working knives. Some of the earthenware vessels and bowls were kept in nets hanging from the roof to protect their contents against vermin. Of interest are three little boxes for unburnt coffee beans: men and women kept in their bags or clothing such boxes with coffee beans to chew occasionally. Czekanowski also collected agricultural tools (for instance, banana knives, bush knives and hoes), clothing (Batoro bark cloth, men's caps, women's decorative tassels and belts), as well as jewellery and combs. He also assembled a fairly complete set of weapons: different kinds of bows and arrows, spears, knives, daggers and three armour plates of leather to protect the breast. Finally there are implements used in initiation (circumcision knives), healing (horns for scarifying) and personal hygiene (toothbrushes, soap and knives for cleaning the nails). In most cases Czekanowski recorded both the use and the local name of the objects. Unfortunately Volume 3 of the *Forschungen* contains neither an account of the Toro collection nor an ethnographic account of the Batoro. Nevertheless we find a history of the Toro kingdom in the chapter on the Bunyoro state.

In the second year of his expedition Czekanowski made anthropological and ethnographical studies of various linguistic and cultural groups in the tropical forest and the adjacent savannas northeast of the Rift Valley, just as he had done in the Upper Nile region. The objects in the Leipzig collection come from the Abarambo, Azande, Mangbetu, Momvu, Logo and Ababua. With the exception of the Momvu collection (80 objects) the number of objects from each group is small. The Momvu, who live partly in the tropical forest and partly in the neighbouring savanna of the eastern Ituri basin, cultivate mainly bananas, but also maize, sesame, beans and cassava. In the savanna regions some millet (*eleusine*) is also cultivated. In earlier centuries their social life was focused upon the clan and the chiefdom. By the beginning of the 19th century most of them had been subjugated by Azande and Mangbetu groups invading from the savannas in the north and northwest. Czekanowski's chapter on the Momvu in Volume 5 of his *Forschungen* contains material on their material

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<sup>21</sup> Cf. Czekanowski 1917, pp. 50ff., 57.

<sup>22</sup> Czekanowski 1958, p. 17.

culture, which is dealt with under the headings “Settlement”, “Household utensils”, “Weapons”, “Musical instruments”, “Clothing, jewellery and personal hygiene”, “Occupations and food”.<sup>23</sup> Here we find many objects very similar to those in the Leipzig collections, including a woven front door, stools for men and women carved in the style of the Mangbetu, woven mats, travelling bags, baskets, caps for men and the characteristic bush knife and insignia with a circular widened blade.<sup>24</sup> The collection also comprises three Momvu bamboo sleeping benches,<sup>25</sup> which have a frame with four legs inserted. This type, adopted from the Mangbetu, was used in Czekanowski's time by the Momvu and the Mangbele. Thus in many cases the material culture of the Momvu and other neighbouring groups indicates cultural assimilation. The originally Bantu-speaking Mangbele are today counted among the Mangbetu group as a result of their linguistic and cultural assimilation. They came from the west and migrated into the Momvu region.

Among other Momvu objects is an interesting series of jewellery plates belonging to women's loincloths.<sup>26</sup> They are fine works of grass and banana leaf with geometric decorations. Attached to the loin-cord women wore in front a semicircular and at the back an oval plate. This kind of jewellery the Momvu adopted also from the Mangbetu. But the older Momvu type - a tassel of long fine twisted black threads - was also worn in Czekanowski's time.

From other ethnic groups of north-eastern Congo the collection contains a set of Zande spears with differently decorated tips and a very well preserved elliptical shield made of thick and solid rattan work, with geometrical decorations on both sides. The handle on the inside is made of a rectangular wooden plate with two parallel empty spaces for gripping.<sup>27</sup>

This list makes it clear that the Leipzig collection does not by any means cover all the groups that Czekanowski visited. There are no objects from the Twa Pygmies, the Pygmies of the tropical forest, the Madi groups – for instance from the Lendu, among whom Czekanowski probably collected some artefacts – or from the Upper Nile region.

The files and publications relating to the German expedition to Central Africa suggest that Czekanowski must have collected more objects than are registered under his name. It remains desirable to clarify the biography of the collection, including its partition between the museums of ethnography in Berlin and Leipzig. Since the collection contains both rare and

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<sup>23</sup> Czekanowski 1924, pp. 400ff, 421ff.

<sup>24</sup> See Czekanowski 1924, pp. 426ff., 436ff., 443f.

<sup>25</sup> Cf. Czekanowski 1924, p. 150.

<sup>26</sup> Cf. Czekanowski 1924, pp. 439f.

<sup>27</sup> Cf. Czekanowski 1924, pp. 36f and ill. 8, 9.

typical ethnographic objects, it deserves to be studied and published. Many of the objects are no longer produced - due not only to the import of European products but also to the endless conflicts in the interlacustrine region.

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## **Czekanowski's Language Data: The Case of Alur**

*Gerald Heusing*

I came into contact with the work of Jan Czekanowski in connection with a linguistic research project which I started three years ago and which is still going on. The objective of my project is to work out a comparative grammar of the group of Southern Lwoo languages which belong to the Eastern Sudanic family within the Nilo-Saharan phylum. The Southern Lwoo languages are spoken in Uganda, Kenya, Tanzania and the Democratic Republic of Congo. Six languages form the main focus of the project; one of them is the Alur language, which is spoken in Uganda and adjacent parts of the Democratic Republic of Congo by approximately 1 million people.

This paper has two parts. In the first I will evaluate Czekanowski's data, and in the second I will demonstrate how these data support one of my hypotheses, which concerns a particular sound change in the Alur language.

### **1 Evaluation of Czekanowski's Data**

The Alur language is not well documented, nor has its grammar been properly analyzed. More research is needed, and for that reason I will soon conduct linguistic fieldwork on the language. For the moment, I must fall back upon already published sources. At the very beginning of the project, I compiled a bibliography for all 6 languages. Concerning Alur, I collected 24 linguistic references - an unexpectedly high number. But before long, disappointment took over: I was forced to subtract 13 works from my list. Some turned out to be manuscripts whose whereabouts were no longer known. Others were simply of little or even no use from a modern linguistic perspective, due to their inadequate notation and/or their briefness.

Czekanowski's contribution to our knowledge of the Alur language is limited to a German-Alur wordlist. The list comprises 191 German entries, for 168 of which Czekanowski provides the Alur equivalent. The wordlist covers nouns, personal and interrogative pronouns, verbs, adjectives, prepositions and numerals. Czekanowski does not explicitly tell us when and where he collected his data: he only writes that he took his notes "on the way" (p. 706: "*Das Verzeichnis der hier aufgeführten Lur-Wörter wurde auf dem Wege notiert.*"). But from what he writes elsewhere in his travel report, we know that he came in contact with Alur

people on two occasions. The first occasion can be dated to December 1908, when he was in the Belgian Congo. He walked on foot through Alur settlement areas from Irumu to Kilo, from there to Mahagi and back to Kilo. On 17<sup>th</sup> December, on his way to Mahagi, he passed a village called Songi; and on the 20<sup>th</sup> he had been in Mahagi. The second occasion occurred some days later in the Ugandan Protectorate. He travelled on the River Nile and spent one night in Sadin and another in Adruggu-el-Bilali. Both places are situated in the area inhabited by the Alur people.

Czekanowski's Alur wordlist gives the impression that he had no or only very little linguistic training. His wordlist is just an appendix to his mainly anthropological and historical statements. In this respect, he joins his contemporaries who had written about the Alur. All of them added wordlists with little linguistic expressiveness to their work, which focussed on other issues. This is true for the Italian explorer Major Gaetano Casati, in whose 1891 publication (German edition) we find an Alur wordlist, as well as for the Special Commissioner to the Uganda Protectorate, Sir Harry Johnston, who published an Alur wordlist in 1904 as part of his overall attempt to describe several aspects of the Uganda Protectorate. Johnston made this point very clear in the subtitle of his work, which reads as follows: "An attempt to give some description of the physical geography, botany, zoology, anthropology, languages and history of the territories under British protection in East Central Africa ...". The attitude of universal scholars or universally educated explorers such as Casati, Czekanowski und Johnston is characteristic for their time, when human knowledge and scientific diversity were still limited. As far as the study of African languages is concerned, an academic discipline in its own right called "*Afrikanistik*" had come into existence in Germany after World War I. But not until almost the middle of the 20<sup>th</sup> century did it become frowned upon for non-linguists to deal with languages without a minimum of linguistic training. It was only after World War II that scientific specialisation no longer permitted such persons to do linguistic research on African languages.

Now, let me highlight some of the deficiencies found in Jan Czekanowski's Alur word list. First of all, Czekanowski did not use phonetic script. Instead he employed Latin letters, mainly in the way they were used in German orthography. His transcriptions are, therefore, phonetically inadequate. As an example, he did not differentiate between open and closed vowels, although both are phonemes in Alur. Table 1 offers two examples, comparing Czekanowski's data with those found in the Alur dictionary published by Ukoko et al. in 1964, for which the data were recorded between October 1959 and June 1960.



**Table 1**

	Czekanowski 1924	Ukoko et al. 1964
<i>foot</i>	tyelo	ty(;'l(;' <i>foot</i> tye;'lo;' <i>to jut, project</i>
<i>he</i>	en	(;'n <i>he</i> e;'n <i>this one, these ones</i>

Second, Czekanowski did not mark tones, although they play an important role in the lexicon and grammar of Alur. Thus, his entries are not adequately transcribed, and some are even ambiguous, as the examples reproduced in Table 2 show. High tones in examples from the 1964 dictionary are marked with an acute accent, low tones with a grave accent and mid tones with the help of a macron.

**Table 2**

	Czekanowski 1924	Ukoko et al. 1964
<i>chin</i>	tik	t(;'k <i>chin</i> t(;'k <i>bad smell</i>
<i>honey</i>	kitsh	k(;'c <i>honey</i> k(;'c <i>sadness</i>
<i>pig</i>	kul	ku;'l <i>pig</i> ku;'l <i>inclination</i>
<i>river</i>	kulo	ku;'lo;- <i>river(bed), valley</i> ku;'lo;' <i>to incline</i>
<i>tooth</i>	lak	la;'k <i>tooth</i> la;'k <i>root, cause, origin</i>
<i>up</i>	malo	ma;'lo;' <i>up</i> ma;'lo;' <i>to grill</i>

A third deficiency is the recording of morphologically complex forms instead of word roots, e.g. verbs with subject prefixes and nouns with possessive suffixes (see the examples in Table 3).

**Table 3**

	Czekanowski 1924	Ukoko et al. 1964 Knappert 1963
<i>to eat</i>	atshamu	ca;`mo;` <i>to eat</i> a;`-ca;`mo;` <i>I ate</i> a;`-ca;`mo; ( <i>I eat</i> a;`-ca;`mo;` <i>I usually eat</i>
<i>to see</i>	aneno	n(;`n(;` <i>to see</i> a;`-n(;`n(;` <i>I saw</i> a;`-n(;`n(; ( <i>I see</i> a;`-n(;`n(;` <i>I usually see</i>
<i>ear</i>	iti	(;`th <i>ear</i> (;`th-(;` <i>my ear</i>
<i>shoulder</i>	goke	go;`k <i>shoulder</i> go;`k-(;` <i>his, her, its</i> <i>shoulder</i>

Concerning the last two examples it would be unfair to blame Czekanowski too harshly. Speakers of Alur and closely related languages have a semantic concept of body parts which differs from that in European languages. They find it extremely difficult to consider body parts as being independent elements. For them, body parts cannot be separated from the person or animal they belong to. Hence, body part nouns have to be used accordingly, i.e. together with possessive suffixes, even when spoken in isolation. I made the same experience during my own fieldwork with speakers of Kumam and Adhola.

A fourth deficiency is the fact that Czekanowski's wordlist is incomplete. 23 Alur words are not listed. The meanings of some of them - for instance, *be sick*, *be sweet*, *be fast* and *be hot* - are rather abstract and need some explanations if you work with an informant. Such omissions are understandable if the researcher has only limited time and no *lingua franca* for communication with the informant. On the other hand, the absence of words with very concrete meanings, such as *arm*, *hand*, *hair*, *head*, *lips*, *ring* and *lower leg*, is more difficult to excuse.

## 2 How Czekanowski Helped Me

Beside these deficiencies and against my own expectation, Czekanowski's Alur wordlist proved to be useful to me. In the remainder of this paper I would like to demonstrate this.

One aspect of my research concerns the comparison of sounds used in the languages in question. In this respect, the Alur language exhibits one peculiarity. It is the only language in which labiovelar sounds are used. A labiovelar sound is a double stop produced with the lips being together and the back of the tongue touching the velum at the same time. Voiceless labiovelars are usually written as a sequence /kp/ while voiced labiovelars are written /gb/. In Alur, the voiced labiovelar is sometimes produced with implosive airstream. In this case, /b/ is written as a hooked letter /ɓ/. The minimal pairs reproduced in Table 4 testify that labiovelars in Alur can distinguish the meaning of words (and linguists therefore call them phonemes):

**Table 4** (data from Ukoko et al. 1964)

/kpa;`ya;`/ <i>jest, joke</i>	vs.	/ga;`ya;`/ <i>sweet sorgho</i>
/kpe;`ro;`/ <i>to distribute, etc.</i>	vs.	/ge;`ro;`/ <i>to be rude</i>
/gɓ;`yo;`/ <i>to bark</i>	vs. /dhu;`yo;`/ <i>to pierce</i>	vs. /ru;`yo;`/ <i>to cry (down)</i>

It is one thing to discover that Alur is the only Southern Lwoo language to have labiovelars; to provide an explanation why this is so is another. My explanation goes along the following general lines: we are dealing here with a recent historical sound change in which labialized velars (i.e. velar sounds produced with rounded lips; they are written here as sequences /kw/ and /gw/ respectively) developed into labiovelars. On analogy with this basic historical sound change, pure labials like /p, b, w/ and also the pure velar stop /k/ developed into labiovelars. The basic historical sound change and the analogical sound changes are displayed in Table 5:

**Table 5**

I. Basic sound change	<p>labialized velars changed into labiovelars, i.e.</p> <p>/kw/ &gt; /kp/</p> <p>/gw/ &gt; /gb, g≡/</p>
II. Analogical sound changes	<p>pure labials and velars changed into labiovelars, i.e.</p> <p>/p/ &gt; /kp/</p> <p>/b/ &gt; /gb, g≡/</p> <p>/w/ &gt; /kp/</p> <p>/k/ &gt; /kp/</p>

Having given this brief explanation for the existence of labiovelars in Alur, I will now go into a little more detail. In the north and west, Alur is surrounded by Madi, Lendu, Ndo and Lugbara (all of them belonging to another genetic stock of languages called Central Sudanic), which display labiovelars as elements of their sound systems. Lexical borrowing from these languages certainly supported - and may indeed have triggered - the development from labialized velars into labiovelars in Western Alur. One example of a Lugbara loanword which entered the Alur lexicon is /a;`gba;`ka;`/ *big basket* (cf. Lugbara /a;`gba;`ka;`/ *large basket*)<sup>28</sup>.

On the one hand labialized velars can be reconstructed as Proto-South-Lwoo sounds; on the other the development of labialized velars into labiovelars is phonetically plausible and has so far been testified for many African languages of different genetic affiliation. It has for example been attested for the Bantu languages Kinyarwanda and Kirundi, as well as for West Chadic Gwandara and Eastern Nilotic Kakwa, to mention just a few. One argument for such a development in Alur posits the coexistence of voiceless labiovelars and voiceless labialized velars in the western variety of the language described by Ukoko et al. (1964). In their dictionary we find the main forms and by-forms for identical words, as given below:

/kpa;`sa;-kpa;`sa;-, kwa;`sa;-kwa;`sa;-/ *fruitless, in vain, exhaustion*

<sup>28</sup> Instead, Acholi uses the word /a;`du;-ku;-/.

/kpa;`ya;`, kwa;`ya;`/ *jest, joke*

/kp(;`, kw(;`/ *with difficulty*

/kp(;`, kw(;`/ *on the level, evenly, smoothly*

However, the coexistence of voiced labiovelars and voiced labialized velars could not be proved on the basis of Ukoko et al. (1964).

A comparison of Western Alur with its eastern counterpart (described by Ringe 1948) reveals furthermore that labiovelars are only found in Western Alur and that they sometimes correspond to labialized velars in Eastern Alur (see Table 6).

**Table 6** (data from Ringe 1948)

/kwaya/ <sup>29</sup> <i>joke</i> cf. above
/kwE/ <i>in vain</i> cf. above
/kwe/ <i>absolutely</i> (may correspond to Western Alur /kpa; (/ <i>abruptly, curtly, absolutely,</i>
<i>openly, publicly</i> )
/agwata/ <i>gourd, bowl, cup</i> (may correspond to Western Alur /a;`gbu;`, a;`gbu;`mu;`/
<i>middle size</i>
<i>jar</i> )

Convincing examples are /kwaya/ *joke* and /kwE/ *in vain*, whose equivalents in Western Alur have already been mentioned in the preceding paragraph. A less good example is /kwe/ *absolutely*, which may correspond to /kpa; (/ *abruptly, curtly, absolutely, openly, publicly* in Western Alur. An indication of a similar development regarding the voiced labiovelars could be the word /agwata/ *gourd, bowl, cup*. In Western Alur we find /a;`gbu;`, a;`gbu;`mu;`/ *middle-size jar*, and there may be an etymological connection between both forms, although I cannot explain the other sound changes in a systematic way.

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<sup>29</sup> Tones are not marked in Ringe (1948).

The hypothesis of a development from labialized velars into labiovelars in Western Alur is furthermore supported by comparison with other Southern Lwoo languages, as shown in Table 7:

**Table 7**

Western Alur (Ukoko et al. 1964)	Acholi (Crazzolara 1938)	Kumam (G.H.)	Lango (Noonan 1992)	Proto-South- Lwoo (G.H.)
/kp(;', kw(;'/ <i>with difficulty</i>	/kw(;-(;-/ <i>in vain</i>		/kw(;'/ <i>in vain</i>	
/g <u>ɛ</u> u;`yo;` <sup>30</sup> <i>to bark</i>				*/gwe;`yo;`/ <i>to bark</i>
/a;`kpa;`la;`kpa; `la;`/ <i>strychnos</i> <i>innocua</i> <i>(kind of tree)</i>		/a;`kwa;`la;`kwa; `la;`/ <i>strychnos</i> <i>innocua</i>		

For example, the already mentioned Western Alur word /kp(;', kw(;'/ *with difficulty* is found as /kw(;-(;-/ *in vain* in Acholi and as /kw(;'/ *in vain* in Lango. And Western Alur /gɛu;`yo;`/ *to bark* can surely be traced back to Proto-South-Lwoo \*/gwe;`yo;`/ *to bark*. This sound change (including a phonetic transformation from explosive to implosive) may have been triggered by the Lugbara equivalent /gb(;`/ *to bark*. Compare also Western Alur /a;`kpa;`la;`kpa;`la;`/ and Kumam /a;`kwa;`la;`kwa;`la;`/ both denoting a kind of tree with large edible fruits (*strychnos innocua*).

The number of words containing labiovelars which one can detect in the 1964 dictionary is very limited (altogether they comprise 46; 11 of them are ideophones), and the distribution of labiovelars is restricted to root initial position. Due to this, I argue that the basic change from labialized velars into labiovelars as well as the analogical changes from

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<sup>30</sup> Cf. Lugbara /gb(;`/ *to bark*.

pure labials and velars respectively into labiovelars is a recent development, which presumably started some time in the first half of the 20<sup>th</sup> century.

And now, at last, Czekanowski (1924) comes into play, because he supports this impression. In Czekanowski's wordlist for Western Alur (recorded in December 1908, as mentioned above) we find the preposition /wengle/ *near* with an initial pure labial sound. In modern Western Alur as quoted by Ukoko et al. (1964) the preposition has become /kpe; 'Nge; 'le; / *next, near, close by*. Recall that the data of Ukoko et al. were recorded between October 1959 and June 1960. In other words: Czekanowski's entry proves that the analogical sound change from a pure labial sound /w/ to a labiovelar sound /kp/ must have happened relatively recently - between 1908 and 1960.

What final conclusion can we draw concerning the usefulness of Czekanowski's language data for Alur (and maybe also for other languages)? Despite all their deficiencies, we should always take their existence into account, because they represent a particular historical state of a language which has no written tradition of its own. And sometimes, out of the blue, they may become helpful. When this happens, we are grateful to Czekanowski for his legacy.

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## **Czekanowski's Wax Cylinder Recordings from East Africa**

*Susanne Ziegler*

The Berlin *Phonogramm-Archiv* was founded in September 1900 by the psychologist Carl Stumpf with the phonographic recordings of a Thai theatre ensemble. The institution's aim was to document and study musical cultures outside the known field of western music. Until 1922 the archive belonged to the University of Berlin; then it was transferred to the Conservatoire of Music, and since 1934 it has constituted part of the Museum of Ethnography (recently renamed the 'Ethnological Museum'). Of particular value are the historical recordings on Edison wax cylinders from the period 1893-1954, which were added to UNESCO's list 'Memory of the World' as part of the world's cultural heritage in 1999.<sup>31</sup>

Among more than 16,700 original wax cylinder recordings the archive contains about 2,200 which were recorded before the First World War in Africa. Scholars from various disciplines, as well as colonial officials, military officers and missionaries agreed to take part. Through the archive's good relations with the Royal Museum of Ethnography it was possible to persuade numerous expeditions to take a phonograph with them and make musical recordings.

From the archival material<sup>32</sup> it appears that in 1907 Jan Czekanowski, at that time an assistant at the Royal Museum of Ethnography, received a phonograph together with the

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<sup>31</sup> For the history of the archive see Simon 2000, which includes an article on the collections of wax cylinders (Ziegler 2000). An extensive catalogue with all documents on CD-ROM is in preparation.

<sup>32</sup> Material relating to Czekanowski can be found both in the records of the Ethnological Museum and in the correspondence of the *Phonogramm-Archiv*.

necessary equipment and blank wax cylinders for the proposed expedition to Central Africa.<sup>33</sup> Lack of time led to Czekanowski being "made acquainted with the technique of recording" by Felix von Luschan, although the latter would have preferred to have left this task to the *Phonogramm-Archiv*.<sup>34</sup> The archive's director, Hornbostel, later sent written instructions to await Czekanowski's arrival in Naples on his way to Africa.<sup>35</sup>

Czekanowski's first letter from Africa indicates that he had considerable difficulties with the phonograph.<sup>36</sup> These were of his own making, as he himself admitted, and subsequently he was able to make recordings and send the cylinders in boxes to Berlin,<sup>37</sup> where those with musical recordings were galvanised in the usual manner. In each case several copies were made. Hornbostel used these copies to make transcriptions of the pieces, which were later published as an appendix to the report of the expedition (Hornbostel 1917).

Today the whole collection is available for research purposes, with the exception of five cylinders, which are presumed to have been broken on the way to Berlin. It contains the earliest examples of music from the extreme northwest of German East Africa. Altogether there are 84 cylinders marked "Czekanowski Ruanda", 83 of which were described in the accompanying journal, which gave the place of recording, the ethnic affiliation of the performer(s) and the titles of the pieces. Although the *Phonogramm-Archiv* was primarily interested in musical recordings, many collectors also made recordings of speech. Thus the Czekanowski collection includes six cylinders with examples spoken by Tutsi, Hutu and Nyambo, all preserved in the original format (i.e. not galvanised). The majority of the recordings are songs performed solo or in parts, most often in antiphony. Only in two cases were musical instruments - flutes - recorded, and only one such cylinder has survived (Cylinder 36).

The largest group among the wax cylinders is made up by the Hutu songs - of men and of women - recorded in Ruasa (Mulera) or in Kagbaye. From a musical point of view they are not particularly outstanding. Of particular interest are the recordings made at the court of King Musinga, which were also documented in photographs. Whilst the songs of the Tutsi did not particularly impress Czekanowski, those of the King's court minstrels, who were Twa, struck him as "melodious and savage".<sup>38</sup> Hornbostel too noted the unusual kind of polyphony (Hornbostel 1917: 412). Czekanowski tells us (1917: 255) that he played to the King not only

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<sup>33</sup> On Czekanowski and the museum see Stelzig & Adler 2000.

<sup>34</sup> von Luschan to *Phonogramm-Archiv*, 2 May 1907.

<sup>35</sup> Hornbostel to von Luschan, 10 May 1907.

<sup>36</sup> Czekanowski to Hornbostel, n.d.; reply, 1 August 1907.

<sup>37</sup> Czekanowski to Hornbostel, 25 October 1907.

<sup>38</sup> *melodisch und wild*: Czekanowski 1917: 253.

his own recordings - a procedure which always impaired the quality of the cylinders<sup>39</sup> - but also European pieces; evidently, however, the latter made no impression upon the African audience. Equally stereotype was the reaction of the women: like other Europeans who recorded music in this period, Czekanowski reported (1917: 259) that they laughed when they listened to the songs played on the phonograph.

The texts of the recordings were published by Czekanowski (1917: 355-378). For musicological research, which was generally undertaken by Hornbostel as director of the archive rather than by the collector, not all of the pieces were suitable, since many of the melodies were scarcely audible on account of the poor quality of the recording. Hornbostel published some transcriptions and analyses (Hornbostel 1910, 1917), and later Marius Schneider (1934) analysed some of the polyphonic pieces.

Since then no further studies have been made of these historical recordings. Like other wax recordings of the Berlin *Phonogramm-Archiv* they were evacuated during the Second World War and did not return to the Museum of Ethnography until 1991. As part of a large-scale project devoted to the wax cylinders they have since been copied on to digital sound carriers. So far, however, only a few of them are available to the public: Cylinder 62, which was also included in the set of demonstration recordings put together by Hornbostel in about 1920, featured on a record issued in the USA in 1963;<sup>40</sup> Cylinder 35 was included on the CD produced to celebrate the centenary of the *Phonogramm-Archiv*, with a commentary by Jos Gansemans (Simon & Wegner 2000); and a chapter is devoted to Czekanowski in the multimedia installation "Musical Map of the World" - currently being shown at Berlin's Ethnological Museum -, which Ulrich Wegner created for the Berlin exhibition "Seven Hills - Pictures and Signs of the 21st Century".

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<sup>39</sup> In the instructions it was recommended that the collector should play the recording only once in the field: "It generally gives the natives pleasure and encourages them to produce more". They should refrain from playing the recording more than once, however, in order to save the cylinder from damage: Luschan 1904: 2.

<sup>40</sup> George List & Kurt Reinhard, "The Demonstration Collection of E. M. von Hornbostel and the Berlin Phonogram-Archive", Ethnic Folkways Library FE 4175, LP and booklet.

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## **An Evaluation of Czekanowski's Research on Africa in the Light of Later Research, with Special Reference to Poland**

*Joanna Bar*

In June 1907 the German interdisciplinary expedition organised and led by Adolf Friedrich, Duke of Mecklenburg, set out for Africa. The aim of the expedition was to study systematically the northwestern part of the territory belonging to German East Africa, the Central African valleys between Lakes Kiwu and Albert, as well as the northeastern border areas of the Congo Free State. One of the members of the expedition was a Pole.<sup>41</sup>

Jan Czekanowski, the anthropologist and ethnographer of the expedition, comprised the results of his studies in the five-volume work entitled *Forschungen im Nil-Kongo-Zwischengebiet*, being an integral part of the entire set of publications, *Wissenschaftliche Ergebnisse der Deutschen Zentral-Afrika-Expedition 1907-1908 unter Führung Adolf Friedrichs, Herzogs zu Mecklenburg*, the entire set consisting of the works of the specialist members of the expedition: the topographer, the geologist, the meteorologist, the botanist, and the zoologist.

The expedition was intended to pursue the systematic scientific exploration of an area of Central Africa that was still little known. If we look at the route actually covered by the expedition, we can see that it crossed the entire African continent, having moved between 6° North and 2° South. Thus, the expedition traversed the territories of what are now five countries: Kenya, Uganda, Tanzania, Rwanda and the Democratic Republic of Congo (at that time: British East Africa, German East Africa, the Congo Free State and the French Congo).

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<sup>41</sup> Jan Czekanowski (1882-1965), anthropologist and ethnographer, student of R. Martin. In the years 1906-1910 employed at the Museum für Völkerkunde in Berlin, then, in 1910-1913, the custodian of the African Museum of Anthropology and Ethnography of the Russian Academy of Sciences in St. Petersburg. From 1913 – Professor of Anthropology and Ethnology at Lvov University. An outstanding specialist in Slavic studies and an ethnogeneticist, founder of the so-called Polish school of anthropology. After 1945 a lecturer at the Catholic University of Lublin (1945) and at the Adam Mickiewicz University in Poznań (1946-1960). Full member of the Polish Academy of Arts, and thereafter of the Polish Academy of Sciences. A representative of the historical stream in the anthropological sciences (*An Introduction to the History of the Slavs. Anthropological, Ethnographic, Prehistorical, and Linguistic Perspectives*, 1927; *An Outline of the Anthropology of Poland*, 1930; *Man in Time and Space*, 1934, 1937; all in Polish).

## 1. Publication history

According to Czekanowski, the private objective of the expedition's organiser was a publication documenting the scientific achievements of the members of the expedition. In this manner Adolf Friedrich wished – in his capacity as organiser of the entire undertaking – to secure for himself a lasting position in the history of study of Africa. The research in particular fields was to be carried out by young employees of German scientific institutions, selected by leading professors of Berlin.

The report by Adolf Friedrich himself, *Ins innerste Afrika* (Into the Heartland of Africa), describing the organisation and course of the expedition, was meant to constitute the introductory part of the set of publications, the *Wissenschaftliche Ergebnisse....* The scientific achievements which constituted the contribution of the expedition to science, were to be presented in consecutive volumes by individual scholars. Following the introductory volume written by Adolf Friedrich, the following volumes were planned:

Volume II	a. Topography (Max Weiss)
	b. Geology (Egon Kirschstein)
	c. Meteorology (Walter von Wiese und Kaiserswaldau, edited by Dr. Joester)
Volume III	Botany (Johann Mildbraed)
Volumes IV-V	Zoology (Hermann Schubotz)
Volumes VI-VII	Ethnography and anthropology (Jan Czekanowski)

The *Forschungen...* were initially planned as a separate two-volume publication, encompassing the results of the ethnographic and anthropological studies, within the framework of the series the *Wissenschaftliche Ergebnisse....*, which was to contain the entire scientific findings of the expedition. The enormous amount of the material gathered proved to be decisive, though, for granting Czekanowski the right to publish a further four volumes, i.e. altogether four ethnographic and two anthropological volumes. Unfortunately, at the end of the World War I the package containing the manuscript concerning the craniological materials was lost, and its reconstruction, as it turned out later, dragged on until 1950. Ultimately, therefore, the *Forschungen...* appeared in five volumes. The first two (1917, 1924) contained the ethnographic text materials; Volumes III and V (1911, 1927) contained the illustrative material; and Volume IV (1922) presented a portion of Czekanowski's materials in the domain of physical anthropology, namely his observations on living persons.

The findings contained in the *Forschungen...* are remarkable in terms both of their reliability and their quantity. The first volume, devoted to the study of the interlacustrine region, consists of chapters concerning the organisation of research during the expedition, general information on the region, including its history, the data on the territory of Mpororo (the former southern periphery of the kingdom of Nkole, split between the British and German colonies), as well as 14 chapters concerning Rwanda. A supplement is also enclosed with this volume, containing the music-related materials ("Introductory remarks", "Songs from Rwanda", "Examples of notes and an analysis", "Songs of Bahutu", "Songs of Batutsi", "Songs of Batwa", "Joint characterisation"). This volume is composed of 412 pages of text, 14 pages of musical notation, 186 illustrations, and 4 tables with samples of ornaments made by Rwandan craftsmen.

Volume Two, beginning with general information on the northeastern parts of the Congo Free State, contains the findings of studies conducted among 14 ethnic groups (Azande, Mangbetu, tribes of the Uele basin, Abarambo, Wangwana, Ababwa, Mabudu, Baamba and Mabali, Babira, Bakondjo, the Momvu group, Pygmies, tribes of Madi, Nilotic Hamites, and the Shilluk tribes). This volume includes also, as an appendix, the final summary of the whole ethnographic study, as well as an annex, being a summary of the linguistic research ("Introductory remarks", "Classification of the Bantu languages", "Ancient migrations of the Bantu", "Classification of the Sudanese languages", "General conclusions", "Lists of tribes", "Vocabulary studies"). This volume consists of altogether 714 pages and 294 illustrations.

In these two volumes the author set up the classical scheme of individual chapters, most of which are monographs on tribal communities. Yet as many as 14 chapters were devoted to Rwanda. The state organisation of the Azande and the Mangbetu was treated at greater length, as well. All of the monographs contain detailed information on the territory inhabited by the given people, occupations of the population, types of settlement, material and spiritual culture, family relations, and social organisation. In the case of societies with developed state structures, historical information was provided, on the basis of the oral tradition of these peoples, together with data derived from the scholars, officers, or administration of the individual colonial states.

Volumes III (1911: ethnographic-anthropological atlas of the Bantu peoples of the interlacustrine area, Pygmies, and the pygmoidal Batwa, as well as the tall tribes of the rainforest; 139 tables) and V (1927: ethnographic-anthropological atlas of the society of the Azande state and the Nilotic and Uele tribes; 167 tables) are collections of photographs, most of them taken in the period between May 1907 and April 1908. These presentations of



illustrative information were designed to complement the factual material published in Volumes 1-2. The entire set of 306 plates illustrates various spheres of life of the communities analysed – in particular the types of figures, settlements, individual buildings, occupations of the population, products of material culture, domestic animals, games, dances, body deformations, as well as pieces of landscape. The two volumes are prefaced by a couple of sentences explaining the entire set of photographs. In addition, whenever particular persons appear on the photographs, the text provides additional data on the kinship position of these persons, along with a description of the garments, props, and information on the physical type represented, hypothetically defined by the author. Volume III also contains a chapter presenting a breakdown of the Nile-Congo interlacustrine region in anthropological and ethnographic terms. This short description made it possible for the author to limit the explanations concerning the photographs of particular persons.

Brief outlines of the fieldwork conducted appeared first in German scientific journals. The journal *Zeitschrift für Ethnologie* published a paper entitled "Die antropologisch-ethnographischen Arbeiten der Expedition S. H. des Herzogs Adolf Friedrich zu Mecklenburg für den Zeitraum vom 1. Juni bis 1. August 1908" (Czekanowski, 1909). This paper was primarily devoted to findings in the field of physical anthropology, as well as that of social and linguistic relations. A similar paper, entitled "Ethnographie des Nil-Kongo-Zwischengebiet", appeared in *Petermanns Mitteilungen* in 1912.

Until World War II only two papers devoted to the ethnographic studies carried out by Czekanowski in Africa were published in Polish. The first of these, entitled "Badania w międzyczeczu Nilu i Kongo" ("Studies in the Interlacustrine Region between Nile and Congo"), appeared in *Rozprawy Wydziału matematyczno-przyrodniczego AU* (Czekanowski, 1910). The second was a text of barely a dozen pages, "Ostateczne wyniki badań w Afryce Środkowej w latach 1907, 1908 i 1909" ("The final results of research in Central Africa in the years 1907, 1908 and 1909"), published in 1924 in *Przegląd Geograficzny*. This latter paper, albeit short, is of special importance, since it contains a summary of the author's essential findings.

Of all Czekanowski's works devoted to African studies, however, Polish readers are best acquainted with a fragment of the expedition journal, published under the title *W głąb lasów Aruwimi. Dziennik wyprawy do Afryki środkowej* (Into the Forests of Aruwimi: Journal of an Expedition to Central Africa). This report, though not strictly scientific, contains a lot of interesting observations, makes the reader aware of the realities of the fieldwork, and, moreover, offers detailed information on the nature of the author's research, his objectives, the collections he sent to Europe, and the research conducted by other members of the expedition.

## 2. Czekanowski's impact outside Poland

In order to evaluate the scientific value of this work it is necessary to assess the place of Czekanowski's African studies within ethnographic literature on Central Africa. One should therefore start by comparing his findings with those of earlier studies (in particular, those of G.A. von Götzen, R. Kandt, and F.R. von Parish in Rwanda, of J. David, G. Schweinfurth, W. Junker, and G. Casati, H.M. Stanley, A. Huttereau, and H. Johnston among the Pygmies and the Azande), since these reports constituted, side by side with Czekanowski's studies, the primary source of information basis for the *Forschungen*. It is worth emphasising that the region between the Nile and the Congo had not been explored by any ethnographers until the time of Czekanowski. Hence, we can only make comparison with the records of travellers who were not trained in terms of methodology to conduct field studies. Czekanowski's major contribution can be measured in terms of its usefulness for subsequent studies of the African continent. Any evaluation of his achievement, therefore, must take into account the comments by scholars who conducted studies at later times, notably E.E. Evans-Pritchard, M. Baumann, G.P. Murdock, P. Schebesta, C.G. Seligman, M.W. Rogers, Louis, S. Denyer, S. Łysik, Z. Komorowski, and B. Nowak.

One of the most important achievements of Czekanowski in African studies was the determination of the anthropological attribution of the Batwa. Contrary to the opinion of Richard Kandt, who stated that they were Pygmies, Czekanowski's broadly conducted anthropological studies led him to reject this view and support the observations of von Parish. Anthropological analysis made it possible to demonstrate that the Pygmy element of the Batwa was just one secondary component in the physical constitution of the Batwa, due to later influences:

This conclusion, surprising at first glance, finds a simple interpretation given the assumption that the Batwa, as more isolated and less accessible to outside influence, also in anthropological terms, constitute the relics of the ancient anthropological relations in Rwanda. Assuming that Rwanda formerly had a population more akin to the forest Bantu, then also the Batwa, who provide the information on the ancient relations, have to be closely related to the forest Bantu [...]. The anthropological similarity put apart, Batwa are supposed to be on a par with the forest Bantu in one further, very characteristic custom. They are, at least in the north-west of Rwanda, in the western volcanic group, the cannibals. [...] Batwa, just like the forest Bantu, set traps on their paths and stick poisoned spikes into the ground, hovering close by with poisoned arrows in their hands. [...] Otherwise, in terms of material culture, Batwa differ to only to a slight degree from the Banyarwanda. (Czekanowski, 1917, p.131).

It must be emphasised, though, that it was not until the 1960s that a complete anthropological analysis of the population of the interlacustrine region was not achieved, thanks to the work of

Czekanowski's collaborators, who analysed the African materials he had assembled, using modern methods of anthropological calculation. That is also why this success cannot be directly compared to the studies conducted and published at the turn of the century.

Another valuable contribution of Czekanowski's was to establish a precise genealogy for all the rulers of Rwanda, starting with the half-legendary ones and continuing to the present (1917). He determined the date of origin of the dynasty – during the 15<sup>th</sup> century – and linked the establishment of the state of Rwanda with the migration of the Baganda to the interlacustrine region. Czekanowski supposed that the rulers of the northern parts of the interlacustrine region, harassed by the aggressive Baganda, founded a new state in the south. The publications of Czekanowski concerning the history of the states of this region are the main source for the chapter on "The History of the Interior of Central-Eastern Africa (end of the 16<sup>th</sup> – beginning of the 19<sup>th</sup> centuries)" by Bronisław Nowak, the co-author of *Historia Afryki*, a history of Africa published in Polish under the editorship of Tymowski (1996). Nowak made use, in particular, of the genealogical tables of the rulers of Bunyoro and Toro from the Babito dynasty; of the rulers of Nkore ("Nkole" in the writings of Czekanowski), and of Karagwe from the dynasty of Bahinda, as well as of the rulers of Buganda.

The question of the beginnings of the Rwandan state remains a matter of controversy to this day. The opinions of Alexis Kagame, who believed that these beginnings could be traced as far back as the 11<sup>th</sup> century, have divided contemporary scholars, including those in Poland. These opinions are cited, in particular, by Zygmunt Komorowski, in the publication entitled *Kultury Czarnej Afryki* (Cultures of Black Africa) (Komorowski, 1994, p.114).<sup>42</sup>

According to Komorowski, Burundi is a younger, "brotherly creation" of Rwanda. This is in stark opposition to the findings of Czekanowski, who demonstrated that it was the other way around – Rwanda was in older times a part of the kingdom of Urundi, from which it separated and thereafter expanded, so as to become a much more powerful state than its parent.

It is not possible to ultimately resolve the question of the origins of Rwanda, hidden in the darkness of oral traditions. One is surprised, though, to see a Polish scholar ignoring the findings of his countryman. After all Czekanowski's name is mentioned in the bibliography of the book in which Komorowski's chapter appeared – albeit without any reference to his *Forschungen...* (published in German), nor to his paper "*Feudalne państwa pasterskie Afrykańskiego Międzyjezierza*" ("Feudal pastoral states of interlacustrine Africa").

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<sup>42</sup> Komorowski's information on the founder of the state, Gihanda, does not come from the list of rulers published by Czekanowski but merely repeats the findings of Kagame. He also mistakenly suggests, following Cornevin, that the Hutu were a subordinate class whose dependence upon the Tutsi was to be based on the lease of cattle herds owned by the latter.

According to another Pole, Stefan Łysik, in a paper written in Polish entitled "African Pygmies and the problem of their language" (Łysik, 1960), until around 1914 all of the stunted tribes of Asia, Africa (including Bushmen), and Australia, were classified as Pygmies. As years went by, the notion came to be applied more precisely, upon ethnological as well as anthropological criteria, so that doubt was cast upon the supposed unity of the African and Asian 'Pygmies'. A distinction was also introduced between the pure, authentic Pygmies and the so-called Pygmoids, or mixed peoples. Here is how Łysik (1960, p. 38) defines the role played by Czekanowski in the study of the Pygmies:

This is also largely the contribution – if we mean the Pygmies of Africa – of Jan Czekanowski [...]. Czekanowski was the one who established the precise names by which the individual Pygmy tribes called each other, and created the division into the Pygmies and Pygmoids, taking as the criterion of this division first of all the average height of these men, which amounted to 150 cm; the dwarf peoples exceeding this limit and upwards were classified by him as Pygmoids [Batwa from Ruwenzori and Rwanda – J.B.]. (Łysik, 1960, p. 38) <sup>43</sup>

Side by side with his anthropological findings, Czekanowski's most important contribution to the study of the Pygmies lay in the detailed definition and location of the individual Pygmy tribes within the Ituri forest. Czekanowski used here, for comparative purposes, the observations of his predecessors Stuhlmann, Schweinfurth, Emin Pasha, Junker, Casati, Huttereau, Federspiel, Stanley, and David. He also listed the tribal names of particular groups he had heard of, confronting them with the notes of other scholars. With reference to the 'Pygmy language' he wrote:

The facts stated to date allow us to draw a well-justified conclusion that the Pygmies do not have their own language and speak only the idioms of their former neighbours – the full-height Negroes. It is beyond doubt, though, that they preserved their [idioms'] specific features in a more conservative form. Strong aspiration, characteristic for Pygmy pronunciation, is also clearly heard and noticed by the Negroes. (Czekanowski, 1924, p. 473)

This contrasts with Schebesta's view, based upon linguistic material gathered during four trips to Africa in the period between 1929 and 1955, that the Pygmies living in the Ituri forest in the Belgian Congo had perhaps once had their own language, which might have constituted the basis for a special language group in the basin of the Ituri River. Czekanowski was therefore right when he indicated the different phonetics of the Pygmies.<sup>44</sup>

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<sup>43</sup> Nowadays this limit is set at the average height of 145 cm.

<sup>44</sup> Schebesta stated that the Pygmies spoke the language of their Negro neighbours with a somewhat different pronunciation of phonemes and an admixture of alien vocabulary, without counterparts in the Negro dialects. He drew the conclusion that some elements of the original Pygmy language were preserved, both in the grammar and the vocabulary, but above all in phonetics – treating this as evidence that the Bambuti had once had their own language. Further, he formulated a hypothesis,

With regard to Pygmy religion Czekanowski was cautious:

I could learn nothing about the religious imagery of the Pygmies. My informant, named Barza, would simply state that they had no notions of this kind. (Czekanowski, 1924, p. 941)

Whilst Czekanowski knew that Pygmies did not set up huts for spirits, Schebesta wrote:

The religion of the Pygmies is dominated by faith in the highest god [...]. The worship of ancestors and of the dead is not known to them, but along with the cult of the forest god, magic is also practised. They believe in a largely undefined afterlife. They differ distinctly in terms of religion from all the surrounding Negro tribes, in that they worship one god, named differently by the various tribes, which the Negroes do not do. They worship, on the other hand, their dead. (Schebesta, after Łysik, 1960, p. 47)

In general Czekanowski's monograph on the material culture of the Pygmies is very precise, albeit not very voluminous (25 pages). Whatever he could himself see, measure, draw or photograph is beyond any criticism. The errors arose, when he tried to formulate hypotheses related to the spiritual or social culture. He did not know the language, and so he could not conduct truly participant observation, thus being at the mercy of reports of his not always competent, though carefully selected, interpreters:

Long negotiations were caused, it seems, exclusively by the fact that in distinction to the Pygmies living on the territory of Bambuba, they did not value my red beads and demanded the blue ones. I could not, however, understand their requirements due to difficulties with the adjectives. (Czekanowski, 1924, p. 490).

Czekanowski himself is mentioned in Schebesta's *Les Pygmées du Congo Belge* (1952) as the first anthropologist to study the racial features of the Pygmies in Rwanda and within the Ituri forest.

His findings were also made use of by G.P. Murdock in his work *Africa: Its Peoples and their Culture History* (1956), which refers to the studies of Czekanowski concerning the organisation of the monogamous family of the Mbuti tribes inhabiting the Ituri forest.

Georg Schweinfurth was among the first to make use of Czekanowski's research on the Azande. In the third, extended edition of his book *Im Herzen von Afrika* (Schweinfurth, 1918), he refers to the publication of Czekanowski's "Ethnographie des Nil-Kongo Zwischengebiets", which appeared in the journal *Petermanns Mitteilungen* in 1912. Schweinfurth's footnotes mention Czekanowski four times. Opinions of the Polish scholar are quoted concerning the affiliation of the tribes of Madi, Mayogu and A-Sandeh (Niamniam, Azande), as well as information on their spatial reach. Here the source was a map of the

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based upon the study of the religion of the Bambuti, and more precisely – of the etymology of religious and mythological names in the languages of the Pygmies and their neighbours, that the ante-Pygmy language, existing at some time in the past in Africa, was the root of all African languages.

population of these areas, appended by Czekanowski to his publication of 1912. Schweinfurth regarded the information as entirely conforming to his own observations and emphasised especially the detailed character of the map of ethnic distribution. At this time, of course, the volume of the *Forschungen* referring to these areas (1924) had not yet appeared.

An outstanding analysis of studies conducted among the Azande was carried out in a later period by a well-known specialist in this field, E. E. Evans-Pritchard, in his book *The Azande: History and Political Institutions* (1971). In his introduction he presented and commented upon the work of his predecessors (travellers, administrators, missionaries, and finally the researchers who conducted their observations among the Azande). Evans-Pritchard, who made use of Czekanowski's findings in almost all chapters of his book, emphasised in dealing with the ethnic composition of Zande society that Czekanowski's work was the main source used in his study, and he praised the fact that Czekanowski stressed the ethnic complexity of the Zande state. In the more densely populated southern areas, where Czekanowski had conducted his research, peoples of alien origin constituted the vast majority; they were subordinated, but still not assimilated. In the areas to the south of Bomokandi only a quarter of the inhabitants were people belonging in terms of their race to the politically dominant class of Zande. In his chapter on clans and totems Evans-Pritchard emphasised that Czekanowski had been one of the first scholars to study the institution of the totemic clan. Yet, as he pointed out, due to lack of knowledge of the language and the brevity of his stay, the Pole could not become aware of the diversity of the clan totems. His statement that the number of various totems was relatively small turned out to be far from true, although Czekanowski was often right in his identification of the clan-totem relations.

Evans-Pritchard used Czekanowski's observations (especially those concerning construction, arms, clothing, jewellery, body deformation customs, domestic utensils, musical instruments, oracles, and poisons) many times in his chapter on Zande culture and also drew material from him for his chapters on the organisation of the kingdom and on the royal court, in particular with regard to the relations between ruler and subjects, noting that the ruler, distrustful of the influential Azande, sought support for his authority among the lowest, ethnically alien classes of the population. Likewise, the information supplied by Czekanowski on the difficulty of communication between particular provinces helped Evans-Pritchard to explain the causes of the later weakness of this state.<sup>45</sup> The personal data on the royal family noted by Czekanowski (who also had access to the unpublished notes of A. Huttereau) gave Evans-Pritchard useful information on the territorial distribution of the provinces assigned to particular members of the ruling dynasty at that time.

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<sup>45</sup> The king had stated that he was becoming dependent upon his provincial officers out of necessity and transferring an increasing share of power into their hands.

Thus, these materials became one of the first written sources for the history of the Zande state. The list of clans put together by Czekanowski complemented the list elaborated by Evans-Pritchard himself. Luckily their studies concerned two different regions, which made it possible to obtain a complete image on the basis of mutually complementary data. The British scholar also profited from Czekanowski's information on borrowings and influences, mainly relating to the culture of the Mangbetu.. Only in only two cases did Evans-Pritchard note serious errors: in the way Czekanowski employed the notion of a "slave", which, in his own opinion was solely reserved for prisoners of war and servant maids, and in the statement that only the children born into endogamous marriages were the legitimate Avongara.

In his book *The Position of Women in Primitive Societies and other Essays in Social Anthropology*, Evans-Pritchard tried to settle the question of whether cannibalism was really so common among the Azande as had been widely held. He sharply criticised the opinions of travellers such as Piaggia, Casati, Emin Pasha, Junker, and Schweinfurth,. Czekanowski did not escape this criticism, either, although Evans-Pritchard agreed in principle with his main propositions that cannibalism, widely practised before the Europeans came, never concerned the members of their own tribe, but the prisoners of war and slaves, that is – those who were deprived of public protection. Whereas Czekanowski mentioned two members of the ruling family who were suspected of anthropophagy, Evans-Pritchard stood firmly by the proposition that the ruling family of Avongara never engaged in this practice. Moreover, he maintained that this practice was the result of influence exerted by foreign peoples who had been politically absorbed. Yet it must be emphasised that the Czekanowski's remarks distinguish themselves by their concreteness from numerous fantastic reports of other scholars, who displayed an unhealthy interest in cannibalism, and are in principle in agreement with Evans-Pritchard's conclusions.

One of the co-authors of the German book "*Völkerkunde von Afrika*" (1940), Hermann Baumann, refers four times to Czekanowski. In describing types of settlement within the rainforest he published a drawing of the gable roof of a house based on a sketch by Czekanowski. With regard to the northern Congo, the author emphasised that the eastern parts of the area were well known owing to Czekanowski's highly professional studies. Baumann cited Czekanowski's view of the Mangbetu as having long skulls (artificially amplified), a lighter skin, and a strikingly graceful body. He emphasises the role of the research carried out by Czekanowski and then by Schebesta in expanding scholarly knowledge concerning the Pygmies and their original geographical distribution. Then, in the chapter on the southeastern Sudan, Baumann mentions Czekanowski side by side with Evans-Pritchard, Santandrea and Tucker, as one of the authorities on the ethnic situation between the Nile and Congo rivers.

Data contained in the *Forschungen* were presented in two further chapters of the book by Murdock (1956), mentioned previously. Of the 32 'equatorial Bantu' tribes mentioned by Murdock, the data on seven - Amba, Baabwa, Bira, Budu, Lika, Ndaka with Bali, and Nyari - are taken from the work of the Polish ethnographer, in particular with reference to the cultivation of particular plants, construction of houses, tribal organisation and the universality of circumcision, as well as on marriage. With regard to the spread of pastoralism in the interlacustrine area, on the other hand, Murdock made use of Czekanowski's material on the Konjo and Rwanda ('Bakondjo' and 'Banjarwanda' in the latter's terminology), relating, in particular, to the origins of the three populations inhabiting Rwanda - Twa, Hutu and Tutsi -, as well as to farming, livestock, marriage, house construction and state organisation.

Czekanowski's demographic data and anthropological observations are also referred to by C.G. Seligmann in his book *Pagan Tribes of the Nilotic Sudan* (1965), and drawings from the *Forschungen* were presented also in Susan Denyer's *African Traditional Architecture* (1978). Yet whereas Czekanowski's ample lists of basic vocabulary were used in the *Handbook of African Languages* (1952, 1966) edited by A.N. Tucker, these achievements were not mentioned in the overview of the history of the study of African languages presented in S. Piłaszewicz's Polish-language introduction to African studies, *Wstęp do afrykanistyki* (1994).

Had Czekanowski not been able to elaborate and publish his findings himself, they would probably have met the same fate as those of his contemporaries de Calonne Beaufaict and Huttereau, whose work appeared only posthumously and in an incomplete form.

The kind of research conducted by Czekanowski can be classified as what the British scholar Rivers later called "survey studies" - a stay encompassing a large territory, designed to outline, discriminate, and make precise the problems to be studied, as opposed to the "intensive studies" which became popular only in the time of Bronisław Malinowski. Czekanowski's successors were the British students of Malinowski and Radcliffe-Brown, who conducted intensive fieldwork among a selected ethnic group. As Adam Kuper has shown, the first ethnographic studies on the African continent to be based upon long-term participant observation were undertaken twenty years after Czekanowski had stayed there by Evans-Pritchard in the Sudan and Kenya (1926-1938) and Isaac Schapera among the Tswana (1929-1943), both of whom represented the then dominant functionalist school. Yet, as late as 1971 Evans-Pritchard published a monograph, *The Azande: History and Political Institutions*, which, in the words Kuper, was

ethnohistorical and diffusionist, overfilled with details concerning the origins of totems and free of any sociological analyses. It barely differed from the monographs published at the turn of the 20<sup>th</sup> century. (Kuper, 1987, p. 174)



We should also mention that in the 1950s the interlacustrine area became the object of studies conducted within the framework of the East African Institute of Social Research, based in Uganda. One result was the publication of a collective volume, *East African Chiefs. A Study of Political Development in Some Uganda and Tanganyika Tribes* (1959) under the editorship of the Institute's functionalist director, Audrey I. Richards, a student of Malinowski. Here too Czekanowski's material, collected half a century earlier, turned out to be the most valuable.

### 3. Czekanowski's impact in Poland

Finally I would like to touch on the feeble interest in the African research of Jan Czekanowski in his home country. To this day, the *Forschungen* have not been translated, and the contents of this work remain unknown to Polish readers. A student of Czekanowski's, Jan Mydlarski, wrote in 1954:

The two-year study carried out in the watershed of the Nile and the Congo brings enormous materials, both in the domain of anthropology and in ethnography. These materials became the most important source of our information on Central Africa..." (Mydlarski, 1954, p. 1006)

Yet even Czekanowski's own students did not know the *Forschungen*. Tadeusz Dzierżykray-Rogalski published an article on the organisation of the expedition in 1985, based solely on Czekanowski's Polish-language recollections ("Into the Aruwimi forests: Journal of an expedition to Central Africa"). These recollections became in Poland the virtually sole reference cited in relation to Czekanowski's African research. Everyone deemed it a duty to mention the *Forschungen*, but almost always merely in the form of a short note.

It is not necessary to convince anybody that Polish science made no significant contribution to the nineteenth-century exploration of Africa. The situation of the Polish statehood, deprived at that time of political existence, led Polish scientists to set themselves different research goals. Polish names appear only sporadically in the history of the exploration of Africa, most often in connection with the activities of other European countries. The Poles cursorily mentioned were soldiers of the Foreign Legion, engineers and naturalists. Individual personal initiatives (S. Szolc-Rogoziński, J. Rostafiński, A. Rehman, the missionary A. Majewski) appeared to have been the exceptions confirming the general rule. Nevertheless, Polish scientists, although often with foreign funding, made a significant contribution to the later stages of the exploration of Africa. Side by side with Czekanowski we must mention here R. Stopa and A. Waligórski.

Paradoxically, 19<sup>th</sup> century Polish science was interested in geographical discoveries and in the activities of the colonial powers. This was particularly true for the African

continent, which remained practically unknown until the middle of the 19<sup>th</sup> century. Interest in this domain grew partly as a result of the possibility of observing rapidly the advance of research. Books on exploration and travels were published in Poland, and special journals appeared, like *Kolumb* (Columbus), while existing magazines would establish columns or supplements devoted to news from the expanding world (*Biblioteka Warszawska*, *Kurier Warszawski*, *Tygodnik Ilustrowany*, *Przyjaciel Ludu*). Impressions from the private journeys of the globetrotting compatriots (such as Michał Tyszkiewicz or Stanisław Janicki) were published there, not to mention the famous trip of the Nobel-prize-winning novelist Henryk Sienkiewicz to Zanzibar and to the coast of Tanganyika (1891), which resulted in his well known *Listy z Afryki* (Letters from Africa). Scientific societies invited explorers to present lectures which were then published in the journals of these societies. As early as 1910 one of the first reports from the expedition of Jan Czekanowski appeared in the bulletin *Rozprawy Wydziału matematyczno-przyrodniczego AU* (Annals of the Mathematical-Naturalist Department of the Academy of Arts).

It is hard to explain the lack of interest in Czekanowski's research. Part of the answer lies in the fact that this publication was a scientific work, and not a traveller's column. In contrast to the popularity of folklore in Poland, scientific ethnology and anthropology were limited to a narrow group of specialists. African studies in the scientific sense simply did not exist in Poland at the turn of the 20<sup>th</sup> century. Research conducted on other continents was too far removed from Poland's own problems to survive the events following the regaining of independence after World War I.

In 1913 Professor Józef Nusbaum offered Czekanowski the chair at the University of Lvów. The period during which Czekanowski held the Chair of Anthropology and Ethnology in Lwów saw a growth of interest in anthropological Slavic studies and in the anthropology of Poland. Anthropological maps of Poland were elaborated in Lvów, along with Czekanowski's major works: *Wstęp do historii Słowian. Perspektywy antropologiczne, etnograficzne, prehistoryczne i językoznawcze* (Introduction to the history of Slavs: Anthropological, ethnographic, prehistoric and linguistic perspectives) (1927), *Zarys antropologii Polski* (An outline of the anthropology of Poland) (1930), *Człowiek w czasie i przestrzeni* (Man in time and space) (1934, 1937), as well as the chapters for the collective volume *Człowiek, jego rasy i życie* (Humans, their races and life) (1939). In the interwar years Czekanowski created an anthropological school, which applied specially elaborated research methods and collaborated with archaeology, ethnography, sociology, demography, history and linguistics.

Although in later years Czekanowski often returned to African problems, this was limited to the publication of memoirs from his journey (Czekanowski, 1958a, b) and to the

strictly anthropological studies which elaborated the material brought from the expedition (Volume IV of the *Forschungen*).<sup>46</sup> His studies in physical anthropology were taken up by his students (notably Tadeusz Henzel and Father Czesław Białek). The originally envisaged for the 6th volume of the *Forschungen* was to be turned into a tripartite series of publications under the title "Badania antropologiczne w międzyrzeczu Nilu i Konga" (Anthropological studies in the Nile- Congo interlacustrine region) in *Przegląd Antropologiczny*.

Taking into consideration the decadent phase in which the historical direction in the anthropological sciences found itself, the methodological views of Czekanowski resulting from his studies in Zurich, and the new reality of the reborn Poland, we can easily understand the shift in his ethnographic interests.

Yet it remains unexplained why, given the limited possibilities of conducting ethnographic research outside Europe, neither the opportunity was found nor the wish expressed to present the rich ethnographic material contained in the *Forschungen*. It is certainly true that during the inter-war period Poland published very little research relating to African studies, apart from missionary publications (such as the valuable work of Father Edward Kosibowicz on the Pygmies), and the ephemeral activity of the Maritime and Colonial League. Furthermore, it was awkward at this time to refer to participation in a German expedition. In the same period Czekanowski was actively involved in research and political activity which became the focus of a dispute with German scientists on the origins of the Slavs.<sup>47</sup>

Political reasons lay behind the drastic limitation of scientific contacts with the German research community after 1939. After 1945 an anti-German attitude became an integral component of official Marxist science. Theoretical directions which had played a dominant role before the War, lost influence, and

the discussions between the representatives of various scientific directions in the period after 1945 were gradually phased away in the shadow of the growing pressure from the side of Marxism, when the sides to the dispute were the Marxists and all the rest. (Damrosz, 1996, p. 76).

In the historical-political background to the *Forschungen* there loomed, obviously, colonialism. Who would write then of whipping or of European attitudes towards Africans?

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<sup>46</sup> Two exceptions to this generalisation are the 40-page article "Feudalne państwa pasterskie afrykańskiego Międzyjezierza" (Feudal pastoral states of the interlacustrine area), which appeared in *Przegląd Socjologiczny* in 1961 and dealt with the history and social organisation of the states of this area, and the paper "Struktura etniczna Afryki a nawarstwienia najmłodsze" (The ethnic structure of Africa and the latest stratifications), published in *Lud* (1960).

<sup>47</sup> See the paper by Geisenhainer and Mischek in this volume.

Why, then, take the dusty volumes from the bookcase, if neither the author nor the authorities were interested in them any more? "Colonialism is a shame on humanity. It is inseparably linked with the capitalist system and, similarly as this system, colonialism will disappear from the globe", wrote Jerzy Prokopczuk in the introduction to his book (1962). The situation changed somewhat in the second half of the 1960s, when political correctness allowed people to turn their attention to the Black Continent. It was then that Czekanowski published of the excerpts from his journal – *Into the forests of Aruwimi* – and the majority of his Polish papers in scientific and popular magazines. Although hard to believe, one obstacle to the complete presentation of the *Forschungen* seems to be the language barrier – hence, for instance, the repeated translation of the name of the expedition's organiser as "Adolph Francis". In postwar Poland knowledge of the German language and interest in the German-language scientific literature declined – an irony, since it was precisely Czekanowski's knowledge of German that had made it possible for him to enter the world's scientific literature.

The work of Czekanowski was not, alas, a fully elaborated monograph: rather, it constituted a systematically prepared scientific report from a field study. For this very reason most of the reproaches most often directed in the second half of the 20<sup>th</sup> century against the assumptions, logical errors and speculative theories of the cultural-historical school do not apply to the *Forschungen*. What this work provided were self-consistent historical data, which can still serve as a basis for the study of the history of the states of this region of Africa. It is all the more surprising that in Poland, where for decades no resources were available for ethnographic fieldwork in Africa (even Andrzej Waligórski did his research with British funds), the work of Czekanowski remains unknown.

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## **Czekanowski's Impact on Social Science Research Conducted in French**

*Lidia Meschy*

My main aim in this preliminary study is to indicate what information, observation and analyses of Jan Czekanowski's *Forschungen im Nil-Kongo-Zwischengebiet* have been used by French-speaking scientists. I can hardly pretend to have exhausted the subject, which needs both scrupulous bibliographical research into the documents dispatched in France and Belgium and a thorough analysis of the texts and their many-sided contexts. A further aim of

this article is to explain why Czekanowski was neglected for such a long time, although he was gradually rediscovered in the mid-20<sup>th</sup> century and today can be easily found on the Web.

More perhaps than any other explorer of central Africa of his time, Czekanowski not only described the everyday life of its inhabitants and their traditional customs, but also understood perfectly what was wrong with colonialism. A stay in the bush, in the villages or in the rain forest with porters, peasants, white and black smugglers or colonial administrators had acquainted him with their daily problems and mutual relations, thanks to his pioneering research methods and intelligent mind. But in Africa he had to pay dearly for this success. His close contacts with African people or “subaltern” Belgian officers and colonial officials shocked his stuck-up colleagues, members of the Duke of Mecklenburg’s expedition. They disapproved of his familiarity with the Africans and his unorthodox behaviour in the colonial setting. Czekanowski’s attitudes to the colonial establishment and the white man’s prestige were “politically incorrect” for his day.<sup>48</sup> An odd foreigner for the Africans, a Pole for the Germans, a German for the French, an exotic and cosmopolitan explorer for the Poles – he was a complete outsider in the midst of nationalist Europe. Competition between the colonial powers and two world wars had a highly adverse impact on his scientific career, and later, when his work became better known in Western Europe, the communist regime in Poland banned his books and forbade him to go abroad. Nevertheless, even if his work never met with full success, there was always somebody who made use of an idea, a detail or an observation from his books.

A survey of references to Czekanowski’s books shows that he was quoted in different manners at different times. By the time his major publications appeared in Leipzig, in 1917 and 1924,<sup>49</sup> the Germans had lost their colonies and also much of their passion for the scientific exploration of Africa. During the interwar period, studies concerning Africa depended heavily on colonial policy. The French, British, Belgians and Portuguese concentrated their scientific efforts almost exclusively on their own colonies. To break down this consensus or to cross the border of one’s competence was understood as questioning the very foundations of the colonial empires which had been conquered under conditions of strong competition at the end of the 19th century.<sup>50</sup> Czekanowski was considered a

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<sup>48</sup> Jan Czekanowski, 2001 :121-122, 143, see Bibliography.

<sup>49</sup> *Forschungen im Nil-Kongo-Zwischengebiet* von Dr Jan Czekanowski, Leipzig, Klinkhardt & Biermann, 1917 : t. 1, *Ethnographie, Zwischengebiet Mpororo : Ruanda* (mit einem musikalischen Anhang von E.M. v. Hornbostel) ; 1924 : t. 2, *Ethnographie : Uele/Ituri/Nil-Länder*.

<sup>50</sup> For instance, the so-called “Map of Peoples of Black Africa”, published at the end of the colonial period, stops at the borders of the Sudan, Uganda, Tanzania and Angola, without any explanation of the choice of territories which are *inside* or *outside* “Black Africa”. *Carte des populations de l’Afrique Noire, Notice et Catalogue*, Paris, Documentation française, 1962, 47 pp.

dependable witness of the *indigenous* way of life, and his work – in spite of its purely scientific character – as well as his ethnographic collections, were used more or less directly for pragmatic goals by the German and then the Belgian colonial administrations. On the other hand, the lack of any ethnological theory impeded the spread of *Forschungen im Nil-Kongo-Zwischengebiet* beyond Germany and Belgium. While the British concentrated their efforts on functionalist anthropology, in France, where colonial administrators, doctors and travellers were in charge of ethnographical and social investigations, the first ethnologists began to be trained in 1925, when the Institute of Ethnology was founded in Paris. These two reasons explain why Czekanowski's impact remained confined to the areas he had explored.

In the context of this political situation, Czekanowski's ethnographic investigations were of interest primarily to the Belgians, whose interest tended to focus upon various ethnic and social groups and their mutual relations in the area ascribed to the colonial administration. Nevertheless, they gave privileged credence to the research provided by their own colonial and military officers. Most of the documents concerning Congo and Rwanda from the interwar period consist of the reports and diaries of officials and missionaries<sup>51</sup>, travellers' notes, articles of varying scientific value<sup>52</sup>, and some general books on geography and ethnography, which more or less promoted the work of the administration and clergy in Africa. These books were often compilations of other authors' works, whose original sources were rarely mentioned. Contemporary historians have many problems with recognising which are the observations made by the author himself, and which - appropriated more or less accurately - derive from his precursors. The 650-page chronicle entitled *Le Ruanda*, written by the French canon Louis de Lacger in 1939, is one such compilation. Untypically, it includes eight pages of references, and the bibliography contains Czekanowski's *Forschungen*, but without any indication of the information provided. So it represents nothing more than an independent document, a kind of annex for the lecturer who 'wants to know more'.

In studies published in the strictly scientific domain in Belgium during the last decade of the colonial period, references to Czekanowski's works concerned ethnology and social

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<sup>51</sup> The diaries of the White Father's Missions in Rwanda (White Father's Archives, Rome), concerning the period between August and December 1907, constitute rare written testimony with regard to the Duke of Mecklenburg's and Czekanowski's stay in this country. See in particular: Ian Linden, *Church and revolution in Rwanda*, Manchester, Manchester University Press, 1977, 304 pp., translated and revised in French by Paulette Géraud : *Christianisme et pouvoir au Rwanda (1900-1990)*, Paris, Karthala, 1999, 438 pp.

<sup>52</sup> Special mention must be made of the *Bulletin agricole du Congo belge*, which contains a set of good monographs, periodicals such as *Congo* and *Aequatoria*, and also the series of Mémoires de l'Académie royale des sciences d'Outre-mer et du Musée royal du Congo belge in Tervuren, which later became the Musée royal de l'Afrique centrale.



anthropology. Jacques-Jérôme Maquet, who was at the time in charge of the Institute of Scientific Research for Central Africa, located in Butare (Rwanda), quotes the third volume of *Forschungen* in his writings on power and social relations in Rwanda, especially in *The Premise of Inequality in Rwanda*, published in French in 1954 and in English in 1961. H. Van Geluwe, who had conducted fieldwork essentially in the north-eastern Congo, quotes Czekanowski systematically in his monographs on the Bira group (1957), on Mamvu-Mangutu and Balese-Mvuba (1957), and then on Bali (1960), published at the same time by the Musée Royal du Congo Belge in Tervuren and by the International African Institute in London in the series *Ethnographic Survey of Africa*. Van Geluwe draws upon Czekanowski's observations on the material culture and customs of these groups, comparing them with what he found in other sources. His monographs, written in a concise style, present facts based upon fieldwork without any comments or further analysis.

In the second half of the 20<sup>th</sup> century, when methods of investigation changed and most previous theories collapsed, as did the colonies themselves, Czekanowski's "lack" of theory and hypotheses – the reason why the principal French dictionaries of social anthropology and ethnology never mentioned his name – became a guarantee for the intelligence of what he had written. For instance, he was one of the few scholars who did not talk nonsense about the origin of the interlacustrine kingdoms and their pastoral inhabitants, Tutsi in Rwanda or Hima in Uganda. Today, his *Forschungen* are quoted not only for the ethnographic descriptions of material culture, music, habitations, dress etc., but above all for the sociological and political information, which sheds light on the tragic history of human relations in East and Central Africa.

In 1962 a new collective volume on the interlacustrine kingdoms was published by the Musée Royal de l'Afrique Centrale. The author of the important chapter on Rwanda<sup>53</sup>, the Belgian anthropologist Marcel d'Hertefeldt, knew Czekanowski's work very well. Thanks to him, French-speaking scientists could appreciate for the first time the wide range of subject-matter taken up by Czekanowski in his *Forschungen* : estimates of population and population density in different regions, socio-political organisation and the structure of the kingdom, its territorial scope, centralization of power, taxes, ethnic and cultural differences between various territories, the rural economy, management of space, commerce and crafts, types of architecture, customs and traditions, rituals and many details of everyday life which had since disappeared or escaped the attention of other observers.

Another work of d'Hertefeldt, *Les clans du Rwanda ancien*, published in 1971, includes numerous quotations in German from *Forschungen* with commentary in French. He

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<sup>53</sup> D'Hertefeldt., 1962 : 9-112.

underlines that Czekanowski was the first European who noted the simultaneous presence of Hutu, Tutsi and Twa in the same clans and explained it in terms of the social mobility between the three groups. Another important statement concerns the attempts made by the monarchy to dissolve Hutu social organisation in northwestern Rwanda, a region that still reminded imperfectly dominated by the monarchy during the German expedition's stay in the country. M. d'Hertefelt also emphasises Czekanowski's contribution to the recognition of cultural differences between the north and the south of Rwanda.<sup>54</sup> All these observations help to invalidate the theory of the division of Rwandan society into rigid and separated castes, a concept applied in the description of local social structures by authors who did not take Czekanowski's work into account. A glance into the excellent encyclopaedic bibliography of Rwanda written by d'Hertefelt and Danielle de Lame in 1987 is instructive as well : *Forschungen* is referred to as one of the best scientific works concerning Rwanda published before the Second World War.<sup>55</sup>

The French gained an opportunity to discover Jan Czekanowski through the book of Hermann Baumann and Diedrich Westermann, *People and Civilisations of Africa*, which was translated from German into French in 1948. It served as a kind of Bible for those interested in regions of Africa other than the French colonies.

The French lack of interest for Great Lakes region endured until the 1950s, when a new school of African studies was born in France, enhanced by work done in history, social anthropology and sociology at Section VI of the Ecole Pratique des Hautes Etudes in Paris, today called the Ecole des Hautes Etudes en Sciences Sociales. The paper by the sociologist Georges Balandier, "The Colonial Situation : A Theoretical Approach" (1951), is now considered the seminal article which made it possible "to think the reversal of the world after 1950"<sup>56</sup> - de-colonisation, the emergence of the Third World as a concept, the rise of the new nations, under-development and development. Georges Balandier also questioned the divide

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<sup>54</sup> D'Hertefelt, 1971 : 10, 56-57.

<sup>55</sup> Five bibliographical notes concern Czekanowski's work : "Die anthropologisch-ethnographischen Arbeiten der Expedition S.H. des Herzogs Adolf Friedrich zu Mecklenburg", *Zeitschrift für Ethnologie*, XLI, n° 5, 1909, pp. 591-615 ; *Forschungen im Nil-Kongo-Zwischengebiet*, Band 3 (1911) and Band 1 (1917); "Ethnographie des Nil-Kongo-Zwischengebiets", *Petermanns Mitteilungen*, LVIII, 1, 1912, pp. 22-25 and map; "Pygmäen, Pygmoide, Waldstämme, Bahutu und Hirtenadel im Lichte der quantitativen Methode", *Anthropos*, LVII, 3-6, pp. 434-443.

<sup>56</sup> G. Balandier, "La situation coloniale : approche théorique", *Cahiers internationaux de sociologie*, vol. XI, 1951 : 44-79 ; and *Sociologie actuelle de l'Afrique noire*, Paris, PUF, 1955. See also: J. Copans, "La situation coloniale : relecture", *Cahiers internationaux de sociologie*, vol. CX, 2001 : 31-52 ; and "La communauté des ethnologues : le cas des africanistes français et de 'leurs objets' face à la décolonisation", in N. Marouf (dir.), *Identité – Communauté*, Paris, L'Harmattan, 1995 : 99-126 ; André Burguière, "Anthropologie historique" in J. Le Goff, A.R. Chartier, J. Revel (eds), *La nouvelle histoire*, Paris, Retz, 1978 : 37-61.

between societies which have their history and those which do not. The rupture of interdisciplinary barriers and the co-operation of specialists of many disciplines gave African studies a new impetus. Research was conducted on written and oral documents of different types from various periods, along with field research, which took into account not only the chiefs and powerful men but also all those who depended on them - peasants, women, merchants etc. Nevertheless, the young French researchers who worked in Rwanda in the late 1960s and early 1970s did not know German and could not read Czekanowski's books. For some subjects this was a considerable disadvantage.<sup>57</sup> On the other hand, Czekanowski's work became an important source of information for two French historians of the interlacustrine region, due to their good knowledge of German. They used the analyses and the observations contained in *Forschungen* very differently, reflecting their different conceptions of history - classical and factual in the case of Bernard Lugan, analytical and critical in that of Jean-Pierre Chrétien.

Lugan devoted his first works to markets and commercial networks in Rwanda. In 1980 he translated long extracts from the first volume of *Forschungen*.<sup>58</sup> In his doctoral thesis "*Between the Servitude of the Hoe and the Spells of the Cow: The Rural People of Ancient Rwanda*" (1983) he relied mainly upon the work of Czekanowski (34 quotations and 20 photos), on Richard Kandt's *Caput Nili*<sup>59</sup> and on his own fieldwork. More recently, Lugan has quoted *Forschungen* in his *Histoire du Rwanda de la préhistoire à nos jours*, published in 1998. There is no doubt that he made Czekanowski's works more widely known, mainly as a primary written source of historical information.

Jean-Pierre Chrétien started his investigations in Burundi in the early 1970s and then gradually enlarged his fieldwork to cover the whole of the interlacustrine region. He quotes Czekanowski in a great number of his publications and notably in his analytical and bibliographical study of the rural history of the Great Lakes region (*Histoire rurale de l'Afrique des Grands Lacs*, 1983), a useful guide to the history of agriculture. In his papers Chrétien stresses mostly the value of Czekanowski's contribution in questions that were interpreted wrongly for years, one reason being the ignorance of Czekanowski's observations.<sup>60</sup> Thanks to Chrétien, Czekanowski has become better known also among African French-speaking scientists. In 1977 the Burundian historian, Emile Mworoha,

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<sup>57</sup> See: *Cahiers d'études africaines*, n° 53, 1974, "Le problème de la domination étatique au Rwanda – Histoire et économie", edited by Claudine Vidal.

<sup>58</sup> Lugan B., "Sources écrites pouvant servir à l'histoire du Rwanda (1863-1918)", 1980.

<sup>59</sup> Richard Kandt, *Caput Nili. Eine empfindsame Reise zu den Quellen des Nils*, Berlin, 1905.

<sup>60</sup> J.-P. Chrétien (1985 : 1358) considers Czekanowski the most rigorous anthropologist of the early 20<sup>th</sup> century, unjustly neglected by younger specialists of the Great Lakes region.

referred to Czekanowski in his book about the kings and people of the Great Lakes region. More recently, Chrétien has shown in his *L'Afrique des grands Lacs, deux mille ans d'histoire* (2000) that Czekanowski was right when he formulated statements opposed to hypotheses about the "Hamitic", Ethiopian (Galla), Egyptian or even Asian origin of the pastoral Hima (Uganda) and Tutsi (Rwanda and Burundi). This hypothesis, put forward by J.H. Speke in his *Journey of the Discovery of the Source of the Nile* (1863), was carried on by Harry Johnston<sup>61</sup> and then diffused by missionaries or colonial officials, but is today completely rejected. Chrétien has found in *Forschungen* some interesting observations concerning the myth of the dynasty of Bacwezi in the Bunyoro kingdom (northwestern Uganda), the origins of the Babito dynasty, the inner organisation of the Rwandan monarchy and the relations of social and economic dependence between Hutu and Tutsi. Thanks to Czekanowski it has been possible to rid the history of the interlacustrine region of ideological elements introduced by European adherents of Speke's point of view or derived from the oral traditions of the court, transmitted and commented by Alexis Kagame in his monumental history of the Rwandan kingdom.<sup>62</sup>

If Czekanowski's name became better known during the 1980s, his impact on social anthropology, geography and sociology was less significant, because French specialists in East and Central Africa had access to his work only through partial and imperfect translations. In 1989 I published an article in *Etudes rurales* about old irrigation techniques and agricultural tools used in Rwanda, mentioned by Czekanowski and no-one else. This example and many others, such as Noël Ballif's critical analysis of documents about the Pygmies of Central Africa (1980) or Jos Gensemans' ethno-musicological study, *Les instruments de musique du Rwanda* (1988), suggest that the *Forschungen* still contain a lot of neglected observations which may acquire a new value in the light of recent research. In the course of time, Czekanowski has become an indispensable reference even for non-scientists : Jean-Paul Harroy, the last Vice-Governor of Ruanda-Urundi, mentioned him in his memoirs, published in 1984.

Recent publications concerning the Great Lakes region quote frequently from Czekanowski. Besides Chrétien's *L'Afrique des Grand Lacs* (2000) Czekanowski is mentioned in the book *Une archéologie de la violence en Afrique des Grands Lacs* (2000) by

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<sup>61</sup> Harry H. Johnson, *The Uganda Protectorate*, 1902, 2 vol., London, Hutchinson, 1902; *The Nile Quest*, Londres, Lawrence & Bullen, 1903, 341 pp.

<sup>62</sup> See in particular two books written by Alexis Kagame : *Un abrégé de l'ethnohistoire du Rwanda*, Butare, Editions universitaires du Rwanda, 1972, 286 pp., and *Un abrégé de l'histoire du Rwanda de 1853 à 1972*, Butare, Editions universitaires du Rwanda, 1975, 543 pp.

Maniragaba Balibutsa<sup>63</sup>, a Rwandan philosopher who graduated in Germany. The Belgian historian and anthropologist Jan Vansina, working now at the University of Wisconsin, who did not quote Czekanowski much in his early works, does refer to him in his historical study of political tradition in equatorial Africa, published in 1991 in French, especially in the chapter concerning the inhabitants of Bomokandi-Nepoko basin (northwestern Congo), the Mabudu, the Momvu, the Azande and the Abarambo.<sup>64</sup> He also quotes from *Forschungen* in about twenty cases in his new book, *Le Rwanda ancien* (2001), when he speaks about the gathering of oral traditions, myths of the origin of the kingdom, population, illnesses, nourishment, commerce, relations between social groups, irrigation, the tax system, relations of economic dependence, and cultural, territorial and social variations.

Last but not least, one can find Czekanowski on the Web, quoted for instance by Chrétien in his text about the ideological and political genesis of genocide in Rwanda, presented to the deputies of the French Parliament in April 1998.<sup>65</sup> The fact that the official website of the Government of Burundi refers to Czekanowski in the chapter concerning the population<sup>66</sup> proves the importance of his work for debates about the political situation in the Great Lakes region.

This ever-increasing interest in the work of Czekanowski among French scholars induced me to translate into French the literary version of his expedition diary. Although it had already been published in Poland in 1958 by the Polish Society of Ethnology, it remained unknown in the West.<sup>67</sup> The book, which appeared in France and Switzerland in 2001 under the title *Carnets de route au coeur de l'Afrique - des sources du Nil au Congo*, offers many new insights concerning the internal life of the expedition group and of Czekanowski's caravan, his numerous contacts with Africans and Europeans and many other things that are

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<sup>63</sup> See bibliography and the abstract of Balibuta's book by Innocent Nesengimana : [www2.minorisa.es/innshuti/balibut/htm](http://www2.minorisa.es/innshuti/balibut/htm)

<sup>64</sup> Vansina, 1991 : 215-233.

<sup>65</sup> "La genèse idéologique et politique du génocide: l'histoire d'un ethnisme particulier ". See: [www.lexana.org/f/org/199804jpc.htm](http://www.lexana.org/f/org/199804jpc.htm) : "[...] A century ago, the German said quasi all about this subject: the first explorer, Graf Von Goetzen, writes in 1895, echoing his British predecessor in Uganda, Speke: "the great invasions coming from Abyssinia" are at the beginning of these kingdoms. Twenty years later, his compatriot Jan Czekanowski notes that "the Batutsi immigration rests only upon anthropological conjectures, and nobody knows any authentic tradition about it."

<sup>66</sup> [www.burundi.gov.bi/peuplemen.htm](http://www.burundi.gov.bi/peuplemen.htm)

<sup>67</sup> Jan Czekanowski, *W glab lasów Aruwimi. Dziennik wyprawy do Afryki Srodkowej*, Wrocław, Polskie Towarzystwo Ludoznawcze, 1958, 461 pp. First mentioned in French by B. Jewsiewicki in his paper concerning the place of Africa in East European historiography (University of Lubumbashi, Zaïre, 1974), and then in mine about Czekanowski's expedition, published in *Reue française d'histoire d'Outre-mer* in 1994 (see bibliography).

not reported in the *Forschungen*. Its publication presents a new opportunity to make his work better known both in the academic world and among those who like to read travel accounts.

Wars and political troubles have made it difficult to find copies of Czekanowski's publications in France and Belgium, not to speak of Rwanda and Congo, and this, together with the incapacity of most French-speaking scholars to read German, explains why Czekanowski has been discovered so late by them. No doubt a new edition of *Forschungen im Nil-Kongo-Zwischengebiet* and a translation into English or French would be greatly appreciated not only by scholars working on Rwanda and northeastern Congo, but also by all the Africans who have so long been deprived of this document to their history.

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## **Czekanowski's Impact on African Studies in English**

*Krystyna Muszyńska*

This paper deals with several anglophone texts written by social anthropologists, in which Jan Czekanowski and his research played an important role. I will give some examples of publications which discuss Czekanowski's observations concerning the Rwandan and Zande peoples in the field of economic and socio-political relations - the subject of disputes among Africanists for the last 50 years.

When I asked Dr James Fairhead, my lecturer in the ethnography of East Africa at SOAS, University of London, for his opinion on Czekanowski's *Forschungen*. Fairhead was convinced that important are Czekanowski's comments on ethnicity. Such, in Fairhead's opinion are the 'realities' in Rwanda as described by Polish scientist. Equally, he gives a great deal of information on farming and economy (including irrigation, erosion prevention etc.) Fairhead's comment led me to explore Czekanowski's versatile presence in the anglophone literature of social anthropology.

Evans-Pritchard, who is mainly remembered for his analysis of the Nuer and Azande, refers to Czekanowski in his *The Azande* (1971), a monograph based on fieldwork conducted in 1926-1930. Evans-Pritchard perceives the Polish ethnographer as an independent authority, "providing us with excellent information on the Azande of Congo", as he states in the preface (1971: viii). As is easily discernable in the text, Czekanowski constituted one of the principal sources for the historical reconstruction undertaken by Evans-Pritchard. In his chapter on Zande culture he quotes Czekanowski extensively with regard to borrowings in arts and crafts. Evans-Pritchard often treats Czekanowski's account as the most authoritative of his written sources, showing great respect for his observations. In confronting different points of view with regard to cultural drift between the Azande, Mangbetu and other groups, he attached great importance to Czekanowski's assertions. Further references to Czekanowski may be found in chapters devoted to the ethnic composition of Zande society, to the functioning of the royal court and the kingdom of Gbudwe.<sup>68</sup>

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<sup>68</sup> To demonstrate that Evans-Pritchard's position towards Czekanowski was not uncritical, I quote one passage: "Czekanowski uses the word 'slave' to correspond more or less to commoner and subject. This is not permissible. The word can only properly be used for servant-girls in large homes and prisoners of war." (Evans-Pritchard, 1971: 230)

In his series of essays entitled *The Position of Woman in Primitive Societies*, which describes the disappearing customs of East African peoples, Evans-Pritchard underlines the value of the record provided by Czekanowski in two articles: one reconstructs the Zande state, another deals with Zande cannibalism - a topic for which Czekanowski was able to provide some evidence.

References to Czekanowski are also found in William Roger Louis' monograph *Rwanda-Urundi 1884-1919*, published in 1963, which compares various data and methods used in Rwanda and Burundi in the period covered. On the basis of estimates provided by Czekanowski as ethnographer of the Mecklenburg expedition, Louis tried to ascertain demographic details for Rwanda:

Apart from the German administration, Czekanowski was the only investigator who provided an explanation of his method of calculation. He estimated the density of population on the basis of the number of huts seen in certain districts. From the area of the district and the number of huts he calculated the number of inhabitants. (1963: 108-109)

Louis concluded that although this method could not lead to precise results, Czekanowski's estimates compared favorably with the calculations made by the local authorities over a period of years.

Another work that quotes the Polish ethnographer is Hellen Codere's *The Biography of an African Society: Rwanda 1900-1960* (1973). Codere refers to Czekanowski only a few times, but apparently appreciates his open view of society as he encountered it in 1907-8. Czekanowski provides evidence concerning changes and tensions while describing a society as it existed at a particular moment in time. In Codere's book Czekanowski's account appears as a vivid report, which has the specificity of an individual experience. That view of Rwandans functioning in a "bundle of relations" seems to correspond to Codere's model of society as a continuous morphogenic process, as opposed to the equilibrium depicted in functionalist ahistorical approaches. She finds the population figures presented by Czekanowski for the years 1907-1909 to correspond quite closely to those of 1956.<sup>69</sup>

While discussing Tutsi political, economic and social ascendancy, Codere uses Czekanowski's account as confirmation of the existence of some poor Tutsi, i.e. men despoiled of property in lands, cows, vassals, when they fell into disfavor with the Mwami or

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<sup>69</sup> Here is how she comments upon the data provided by the Polish ethnographer: "He [Czekanowski] worked mostly in the North Central section of the country where the Tutsi population would be around 10 per cent of the total as he estimated it to be, and where the Twa are very few in number. [...] Though the percentage figures may have shifted up and down they could surely not have done so sufficiently to alter the general picture of an overwhelming majority of Hutu, and a minority of Tutsi, and mere scattering of Twa." (1973:13-14)

a powerful chief or overlord)<sup>70</sup>. This is how Codere finds support for her argument of widening of economic differentials within Tutsi group, which is the way the differences between Tutsi and Hutu became blurred at many points. She quotes Czekanowski again in discussing the way Europeans were perceived by Rwandans<sup>71</sup> and uses his ethnographic data for killings of men by men.

Another anthropologist, Catharine Newbury, seems to value the output of Czekanowski in her *The Cohesion of Oppression: Clientship and Ethnicity in Rwanda, 1860-1960* (1988), a work which, in opposition to the functionalist view of Macquet, deals with historical change. She argues that to understand the politics of ethnicity one must study the changing context within which ethnic interaction occurs. By offering an ethnographic record of the functioning of social institutions at a particular time, Czekanowski provided important material for such a study. Newbury, who focuses on analyses of clientship as a dynamic phenomenon and not a static social “given”, refers to Czekanowski only in her footnotes; but his data concerning lineages and the characteristic of *ubureetwa* service, its nature and function, play a major role in her argument.<sup>72</sup>

In her chapter on the "changing status of corporate kin groups" she examines how changes in administrative structures and the power of chiefs shaped rural class relations, as reflected in changing patterns of clientship. As Newbury says:

Some ten years after the imposition of German rule, Czekanowski described the *ibiletwa* (people constrained to perform *ubureetwa* service) in north-western Rwanda as men who had received land from a chief; they were distinguished from free farmers who lived on land cleared by their ancestors and who had not been forced off their land by Tutsi authorities. (1988:267)

The shift that Newbury observed was that, initially, hill chiefs imposed *ubureetwa* on selected localized lineage groups, and later, as a result of European colonial policies, clientship came to be imposed on individual adult men. So it appeared to be a particularly important instrument

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<sup>70</sup> Czekanowski, as quoted by Codere (1973:37), mentions “some Tutsi in rags and with but a single vassal in attendance coming to pay their annual homage at the royal court”.

<sup>71</sup> Czekanowski in Codere (1973: 39): “When the Europeans first came among the Rwanda they were regarded with scorn by the Tutsi, who expressed their disgust for their color, their hairiness, their smell, and their unfastidious diet, which included such despised food as eggs, by applying such epithets to them as ‘hyenas’ and ‘wild beasts’.”

<sup>72</sup> Newbury’s book attempts to identify long-term changes in the political system which took place in Rwanda from the mid-nineteenth century (just before the reign of Rwabugiri) to 1960, the year of the first communal elections. She explains how in southwestern Rwanda processes of transformation were molded by the growth of state power, and how the interactive impact of these changes affected rural political consciousness, creating the preconditions for revolution.

used by chiefs to divide lineages, which was a step towards altering the relations of power and meaning attached to ethnicity.

John Iliffe's book, *The African Poor: A History* (1987), utilises various sources, among them those written by earliest anthropologists, in a search for records which focus not only on dominant groups but also on marginal ones.<sup>73</sup> Being primarily interested in records which depict poverty as a complex issue, he valued Czekanowski's account of Rwanda and Burundi because his observations were diversified and did not obscure the distinction between the 'ordinary poor' and the 'very poor', and because they were relatively free from preconceptions.<sup>74</sup> Iliffe contrasts Czekanowski's material with that of the French sociologist Claudine Vidal in an attempt to discover whether Africans were impoverished by lack of access to land imposed by political power and how numerous they were. With evidence provided by Czekanowski, he shows that Vidal's view that half the population of Rwanda was impoverished was based upon exaggerated claims by her informants. Following Czekanowski he observes that it could be due to the lack of slaves that the number of poor was important.<sup>75</sup>

Czekanowski thought that poverty was not due to lack of access to land, because land was still amply available in his time; hence power in Rwanda could be exercised much more simply by monopolizing cattle, as was in fact Tutsi practice. Iliffe, however, criticises Czekanowski's view that the Tutsi ruled Rwanda as a conquered territory in which *ubuletwa* (tribute paid partly in kind and partly in labour) was the core of subjection.

In a recent article Johan Pottier (1995) quotes Czekanowski, again with reference to poverty, but this time in order to problematize the question of the formation of Hutu and Tutsi ethnic categories. Pottier highlights Rwanda's socio-political complexity and regional diversity, criticizing the tendency to portray Tutsi-Hutu distinctions in ethnic terms as entirely the invention of European colonizers. He demonstrates that ethnic polarization, and more generally, the politicization of ethnicity, was instituted at least under Rwabugiri's reign. So the recognition of physical difference was by the late pre-colonial era sufficiently

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<sup>73</sup> As Iliffe says, these sources depict the poor as a social category rather than as victims of incapacitation and insecurity. Though that picture may contain some truth, Iliffe explores accounts that were more varied in time and space.

<sup>74</sup> Iliffe cites Czekanowski as his source for the existence of a variety of poor people. Besides landless poor and wage labourers he mentioned bandits, roving girls, victims of warfare and famine, which, as Iliffe observes, corresponds to Codere's account.

<sup>75</sup> Iliffe enumerates four social strata identified by the Polish ethnographer. In fact Czekanowski never described day-labourers as a substantial class, nor did he mention *umucancuro* (day-labourer), as Vidal does a number of times. He referred only to wage labourers, while he talked about a rather undifferentiated *biletwa* class of tributary but land-occupying peasants.

institutionalized to enable a swift transition to ethnic differentiation along full racial lines under European rule.

One of the elements fixing the categorization Hutu-Tutsi was *uburetwa* clientship, which came to be restricted to Hutu. *Uburetwa* (*corvée* labour service and gifts of beer in return for access to land, the most hated of 'feudal' relations) is presented in Pottier's article as an institution importantly enhancing the process. Evidence is drawn from Czekanowski concerning *uburetwa* to demonstrate how different was the reality of precolonial Rwanda. As Pottier says, *uburetwa*, and not *ubuhake*, was the core of Hutu subjection. The assumed equilibrium and reciprocity of Hutu-Tutsi relations in general was based mainly on *ubuhake*, a cattle contract in most cases concluded between elites, which was only one of several institutions.<sup>76</sup> Pottier intends further to underline the value of Czekanowski's material in a forthcoming book.

In his pioneering study, *Agriculture in Congo Basin: Traditions and Change in African Rural Economies* (1967) the American scholar Marvin Miracle classifies traditional agricultural systems in relation to the techniques, environmental conditions and peoples of the Congo. He quotes *Forschungen* in the original as well as the fragments which were translated as part of the Human Relations Area Files in 1960.<sup>77</sup> In the domain of anthropology and migration studies Czekanowski is also quoted by P.T.W. Baxter and Audrey Butt in their book *The Azande and Related Peoples of the Anglo-Egyptian Sudan and Belgian Congo* (1953), which forms part of the International African Institute's series *Ethnographic Survey of Africa*.

All of these texts show that, insofar as Czekanowski's achievement has been recognised at all, it has been mainly thanks to his research in Rwanda and among the Azande, which constitutes only a small - though important - part of his total output on Central Africa. Pottier's forthcoming book raises hope that Czekanowski's importance for the study of other areas will be recognised and find a larger audience. Certainly interest in his work is growing.

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<sup>76</sup> Pottier quotes Vidal's point of view concerning *uburetwa* services and the nature of this institution. He agrees with Iliffe, on the basis of Czekanowski, that Vidal's informants may have exaggerated the size of the much-oppressed class of peasants.

<sup>77</sup> Miracle quotes Czekanowski's with reference to digging sticks, the cultivation of beans and peas, the non-use of fertiliser in areas of Bugoy with rich soil, and the height of terraces (1967, pp. 150, 151). See also pp. 154, 180, 188.

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## Theories of Language and Race in Early Twentieth-Century German Studies of Africa

*Sara Pugach*

Although the title of this paper is broad, it will actually concentrate on the theories of language and race that grew out of a relationship between only two “colonial” scientists: the anthropologist Felix von Luschan and the Africanist linguist Carl Meinhof. While the article will not bear directly on Jan Czekanowski, the main subject of this volume, it should nonetheless help illuminate the intellectual context that he entered when he journeyed to Berlin in 1907. Czekanowski also worked very closely with von Luschan when both were at the Berlin Museum of *Völkerkunde*.<sup>78</sup> His ties to von Luschan were certainly different from those linking von Luschan and Meinhof, who were contemporaries, worked together on at least three occasions and eventually became close enough to take their vacations together.<sup>79</sup>

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<sup>78</sup> Stelzig and Adler 2000.

<sup>79</sup> Staatsbibliothek zu Berlin, Nachlaß Felix von Luschan, Briefe mit Carl Meinhof, letter from Meinhof to von Luschan of 8 September 1904 thanking him for the beautiful day in Millstatt, where von Luschan maintained a country residence.

Czekanowski was younger, had studied under von Luschan, and quarreled with him over various aspects of artifact collection.<sup>80</sup> Even so, understanding the nature of von Luschan's and Meinhof's collaborative research in one area may provide insight into the way in which von Luschan communicated with other scholars.

The issues presented here will, however, also form a more extensive argument about the difficulties that German scholars of the early twentieth century encountered when they attempted to develop theories about Africans, in particular theories about how the various peoples scattered across the continent were to be classified. I will try to show that, while experts from different disciplines – in this particular case African linguistics and physical anthropology – strove to work together to reinforce each other's goals, they did not always produce results that neatly overlapped. Collaboration between practitioners from different fields was, on the contrary, fraught with tension. On an intellectual or theoretical level, the process of defining or classifying Africans was severely compromised by the fact that linguistic and anthropological knowledge or data could often not be reconciled, which made it difficult for scholars to combine the results of their separate investigations and categorize them into tidy conceptual boxes.

One of the problems was, perhaps, that the fields of linguistics and anthropology as they were constituted at the beginning of the twentieth century were both going through rapid change, transforming from sciences of inclusion to those of exclusion. For much of the nineteenth century, the amateur pursuit of African linguistics was practiced largely – if not entirely – by Protestant missionaries interested in compiling dictionaries and grammars that would help advance the cause of Christendom in Africa. Language was a tool, to be used by missionaries seeking to convert indigenous peoples. It was, however, also more: language proved that Europeans and Africans shared a bond that separated them, as it did all of humanity, from the animals. As S.W. Koelle, who was both a missionary and linguistic pioneer in Sierra Leone, stated in an 1851 grammar and dictionary of the Vai language,

I am not aware whether attempts have ever been made to identify the roots of Negro languages with those of other lingual stocks. However that may be, I hope that the subjoined catalogue of compared roots will make the impression, that the sameness or affinity of sounds therein exhibited cannot be accidental, but must have a broader and surer basis. What else can that basis be, but the common humanity which the Negro shares with the Caucasian.<sup>81</sup>

Both Koelle and his missionary colleagues had a vested interest in proving that all people shared mutual origins. If Africans belonged to another species, as contemporary scientists

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<sup>80</sup> Stelzig and Adler 2000.

<sup>81</sup> Koelle 1851: 4.



such as Ernst Haeckel maintained in saying that African languages were closer to the sounds made by monkeys and apes than they were to the language of “Goethe or Schiller”,<sup>82</sup> then missionaries had no reason to preach to them; they were not in the same category as Europeans. If, however, missionaries could show that African languages were related to European ones, they would demonstrate a human bond that gave Africans the same right to civilization and Christianization.

Although the discourse surrounding African philology changed as the nineteenth century neared its close, and the racial divides separating language families were more sharply drawn, a core belief in the undeniable humanity of all people remained. Meinhof would likely have argued against Koelle’s assertion that a comparison of word lists would evince similarities between all categories of language, African as well as Indo-European; he did not, for instance, see a direct connection between Bantu and European languages. Even so, he still considered all languages to have a common root and believed they went through a similar developmental process, transforming from simplistic morphological structures to more complex ones.<sup>83</sup>

While anthropology also had some origins in the missionary tradition, when it was a nascent and undefined discipline at mid-century many of its main German practitioners were doctors or those trained in the natural sciences.<sup>84</sup> In this regard, early anthropologists did not necessarily have the same interest as missionaries in upholding a certain vision of African humanity, in that they were often more intent on collecting data than on converting souls. The subjects did, however, have various collective traits. For one, both were largely grounded in institutions external to the university, African linguistics in the mission, anthropology in scientific societies and museums.<sup>85</sup> Additionally, ethnologists such as Adolf Bastian shared in the belief that there was a certain genius present in all peoples, and that each exhibited progress through individual invention.<sup>86</sup> Even craniology – the physical anthropological practice of measuring skulls in order to compare them across “races” – did not necessarily imply that blacks and whites were irrevocably other; as Benoit Massin has contended, such practices sometimes served to reinforce a discourse of sameness, not one of difference.<sup>87</sup> German anthropology at this stage was, moreover, monogenist and not polygenist, which meant that its practitioners hewed to a belief that all mankind stemmed from the same source,

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<sup>82</sup> Jungraithmayr 1987: 191.

<sup>83</sup> Meinhof 1910a.

<sup>84</sup> Heintze 1999.

<sup>85</sup> Zimmerman 2001; Penny 1999.

<sup>86</sup> Koepping 1983.

<sup>87</sup> Massin 1996.

rather than from separate beginnings.<sup>88</sup> In this, emergent anthropology dovetailed well with missionary linguistics, in that both rejected the idea that Africans were not only another race, but another species.

In the late nineteenth and early twentieth centuries, though, discourses and impressions had begun to shift. Ethnology, for example, was slowly moving away from Bastian's idea of individual invention and towards Friedrich Ratzel's concept of diffusion, which held that cultural products were not innovative to each society and instead were carried – diffused – from one to the other.<sup>89</sup> While diffusionism or the *Kulturkreis* theory that it helped produce – which considered cultures to belong to certain “circles” from which they received their knowledge – is not present in name in Meinhof's theories, it is also evidently not absent.

Moreover, as Massin also demonstrates, the onset of colonialism in Germany had a marked effect on the “racial liberalism” of physical anthropologists. While decrying those who, like Haeckel, would classify Africans as “apes” or call them “animalistic”, they nonetheless assumed a Darwinian evolutionary hierarchy that placed “savage” Africans near the bottom of the ladder, at first culturally, and then by association racially and biologically. Despite their assertion that all people were related and stemmed from the same source, even well-known progressives such as Rudolf Virchow categorized Africans and other colonized people into vastly dissimilar racial groups, which although they did not belong to different species were wholly separate in terms of their position on a developmental scale.<sup>90</sup> Further, even as von Luschan was deemed Virchow's “spiritual” successor, he had close affiliations to colonial societies and organizations, endorsing their goals and acknowledging mental and physical “difference” among blacks and whites.<sup>91</sup>

At the turn of the twentieth century, then, both linguistics and anthropology were turning away from earlier, inclusionary conceptions of humanity and increasingly using race as a marker to divide “civilized” societies from their “savage” cousins. Moreover, in this period the disciplinary boundaries dividing various aspects of African studies – anthropological and linguistic – were also still quite malleable. African linguistics, for instance, had only been formally institutionalized in 1887 with the opening of the Berlin Seminar for Oriental Languages at the Friedrich-Wilhelms-Universität, and even this institutionalization was partial. The Seminar offered a few classes in Swahili that were taught

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<sup>88</sup> Ibid., 86-7.

<sup>89</sup> Smith 1987.

<sup>90</sup> Massin 1996: 97-98.

<sup>91</sup> Ibid., 102-104.

largely to future colonists, not aspiring philologists.<sup>92</sup> There were thus few “experts” in any one subject, even though “professionalization” had begun and some scholars had started to specialize in given areas. Additionally, this meant that Africanist linguistics, physical anthropology, ethnology, geography, and so on were all what Meinhof referred to in 1910 as *Hilfswissenschaften*, “Auxiliary Sciences”: when information collected in one discipline left gaps in understanding, scholars would contribute data from other fields. As Meinhof noted with particular respect to African philology,

in our time of increasingly more particularized studies it is completely impossible for a researcher to master several sciences... This has led to an endeavor to see specialists in various areas mutually helping each other wherever possible. And it is so that African linguistics has also been called upon to act as an 'auxiliary science' for all manner of other disciplines...<sup>93</sup>

And act as an auxiliary science it did, as linguists strove jointly with anthropologists to unravel the “mysteries” of African origins. Especially when issues concerning race were involved, however, disciplinary lacunae were not easily filled, and there was a tendency to conflate anthropological and linguistic knowledge.

The colonial context complicated the problem of classifying Africans even further, as the exigencies of colonialism compelled scholars such as Meinhof to produce totalizing, simplistic images of the indigenous populations in Germany’s protectorates. Africans were, in other words, slotted into well-defined categories such as “Hamitic”, “Bantu”, or “Sudanic”. In this schema, which well-predated Meinhof but which he greatly refined and sharpened, the Hamitic were considered the most culturally advanced, the Bantu came second, and the Sudanic were last, seen as the most “primitive” people in Africa. These labels, as we shall see, carried both linguistic *and* anthropological – or racial – connotations. But they also claimed immense practical value for Germany’s imperial project; once German colonists came to understand the differences between these groups, it would be easier for them to acquire a very precise understanding of their social structure. This could be the case even when colonizers were encountering specific African groups for the first time. Once a colonist knew that a certain people was “Bantu”, for instance, he or she would have a good idea of what to expect from both its language and its culture. As Meinhof commented in a series of articles he wrote for the *Jahrbuch über die deutschen Kolonien*, linguists were in the process of “mapping” all

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<sup>92</sup> Geheimes Staatsarchiv Preußischer Kulturbesitz Rep 76Va Sekt. 2 Tit. 1, Nr. 28 Bd. I, Acta betreffend das Seminar für Orientalische Sprachen bei der Universität zu Berlin, vol. I, vom Februar 1885 bis Oktober 1887, “Begründung des Gesuches betr. Aufnahme der wichtigsten Bantusprachen in den Lehrplan des Orientalischen Seminars”, n.d., ca. March 1887.

<sup>93</sup> Meinhof 1910b: 113.

Africa through its languages. When they had finished, he believed Germans would “know” the inhabitants of their colonies effectively enough to control them:

the surprising amount of new discoveries about the native mind is not only important because it generally teaches us to have a higher regard of (African) culture, but also because it helps us to recognize the different nature of various people. What is correct for one group might be wrong for another...<sup>94</sup>

Coming to “set” conclusions about the ways in which African languages, cultures, and physical characteristics were to be ordered was not easy, especially when it was also thought that African groups had “mixed” over time. Yet regardless of this ethnological and linguistic jumble, Meinhof was still confident that German scholarship could uncover the “original condition” of Africa’s “races”,<sup>95</sup> especially if experts were able to determine overlapping conjunctions between the grammatical and physical data that they had separately collected.

In the rest of this discussion, I propose to examine the problems associated with Meinhof’s and von Luschan’s interdisciplinary efforts. I will show that contemporary scholars were cognizant of the perils of transposing arguments made in one context into another, even as they still insisted on doing so. As “colonial” sciences continued to develop in Germany and assume places in schools such as the Berlin Seminar for Oriental Languages and the Hamburg Colonial Institute, there was an increasing temptation to create master theories of racial development, both in Africa and elsewhere; for his part, Meinhof indeed believed that reaching conclusions about African origins would help Europeans find their own roots, as certain African groups might provide “links” or “bridges” to European prehistory. And when there was not enough evidence in his own subject, African linguistics, his connections to the colonial educational establishment would allow him to support his claims by using data that had been collected elsewhere.

## **1. Interdisciplinarity and Africanist Research**

The methodological approach of combining data from different academic fields to reach conclusions concerning African history is not necessarily problematic. Since the 1970s scholars of Africa, such as the archaeologist David Phillipson<sup>96</sup> and the linguist Kay

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<sup>94</sup> Meinhof 1909.

<sup>95</sup> Meinhof 1913.

<sup>96</sup> Phillipson 1993, 2. Here Phillipson stresses the importance of archaeology to the historical enterprise, maintaining that “...the archaeologist’s interpretation of technological skills or economic practices, particularly hunting, agriculture, and the herding of domestic animals, will generally be far more complete and reliable than those that can be obtained by other types of research.”

Williamson,<sup>97</sup> have emphasized the importance of drawing on information from diverse academic fields when reconstructing early African history. They have also acknowledged that there are difficulties with retracing the exact footsteps of the African past. The written sources available are often simply not as extensive as they are for other areas of the world, and so Africanist historians must sometimes rely on linguistic, oral, archaeological, or even botanical data. For instance, the historian David Schoenbrun has demonstrated how linguistic, ethnographic, and other sources can play a role in historical reconstruction, asserting that, in the area of the Great Lakes

ancient history not only undergirds the modern, but still lives, today, in the laying out of a banana garden, the pounding of barkcloth, the social therapy given to an AIDS sufferer, even in the importance of the crested crane in Uganda's coat of arms.<sup>98</sup>

Moreover, it is not only Africanists who have argued for the inclusion of non-documentary evidence in historical writing: the medievalist Aron Gurevich, for instance, has contended that anthropological sources can prove crucial to the study of early Europe.<sup>99</sup> In other words, for African as well as for other histories, in the absence of what western historians would consider more “traditional” literary proof, other sorts of artifacts, linguistic and material, can be of great assistance to historical research.

Meinhof and von Luschan had already identified the need to use information from disparate disciplines in their studies of Africa around 1900, and began to discuss issues surrounding the conjunction of linguistics and anthropology in a correspondence that lasted from this period to von Luschan's death in 1924. At the beginning of their letter exchange, von Luschan was already a relatively well-established anthropologist in Berlin, whereas Meinhof largely stood outside academic circles as a pastor in the Pomeranian town of Zizow, now in eastern Poland. Since the 1880s, however, Meinhof had also been studying African languages. As an ardent friend and supporter of the Protestant mission and a faithful believer in the German colonial project, he had turned to the subject in order to bolster the success of both.<sup>100</sup> With the encouragement of the missionary and Africanist linguist Carl Büttner – who was also the first Swahili teacher at Berlin's Seminar for Oriental Languages – Meinhof went on to compile a comparative Bantu dictionary in 1895 and a *Grundriß der Bantusprachen* in 1899. Despite their differences in academic rank – which closed significantly once Meinhof was employed by the Berlin Seminar for Oriental Languages in 1903, and later assumed

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<sup>97</sup> Williamson 1993: 139.

<sup>98</sup> Schoenbrun 1998: 19.

<sup>99</sup> Gurevich 1992.

<sup>100</sup> Staatsarchiv Hamburg (hereafter StaH), 361-1 Hochschulwesen, Dozenten- u. Personalakten IV, 673, Meinhof, Carl, 23.7.1857, Carl Meinhofs Biographischer Bogen and Lebenslauf.

Germany's first chair in African language studies at the Hamburg Colonial Institute in 1909 – it seems that von Luschan showed a marked interest in Meinhof's work, and the pair shared many of the same intellectual curiosities. Both were concerned with identifying the similarities and differences across African ethnic groups and defining what made someone "Bantu", "Hamitic", or "Sudanic." Time and again, when Meinhof was writing to von Luschan, he drew attention to his research on these three groups of languages and often mused not only about how they were connected to each other, but also about their possible relationships to more far-flung languages, such as Old Syrian or Mongolian, for example questioning whether a "mixture of blacks with the Mongolian race"<sup>101</sup> had influenced racial "types" in the Near East.

Over the course of their long relationship, Meinhof and von Luschan collaborated on three different occasions, in 1905, 1906, and 1912. The subjects they tackled were the "Hottentots" (Nama) of German Southwest Africa (Namibia); a troop of traveling pygmies from Central Africa; and, finally, the languages of the "Hamites", an ethnic category that Meinhof and von Luschan considered "civilizationally advanced" in comparison to other African groups, and which they felt would present them with the most problems when it came to reconciling language and "race."

While the methodological approaches that Meinhof and von Luschan employed seem to resemble those of later scholars in some ways, there is of course a marked difference between their research and more current studies. Present-day intellectuals are interested in reconstructing historical processes, such as the movements of people across the African continent, in order to uncover sequences of past events and explore how various groups arrived at their present locations. The research of contemporary Africanist linguists, anthropologists, archaeologists, and historians is certainly not without its own political and social agendas, and it would be silly to argue the contrary. However, more recent research is still fostered under historical circumstances very different from those encountered by Meinhof and von Luschan, whose scholarly projects were consumed with issues of race and racial classification. Indeed, Joseph Greenberg, an American who became one of the most prominent Africanist linguists in the field during the 1950s, lambasted Meinhof for his reliance on racial categories to support linguistic classifications. After proposing a new way to organize African languages in 1966, Greenberg said:

If the linguistic analysis presented here is correct, then much of what has hitherto been standard physical anthropology and reconstructed culture history in Africa is in need of reconsideration. The vagueness of the use of the term Hamite and its extension as a

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<sup>101</sup> Staatsbibliothek zu Berlin, Nachlass Felix von Luschan, Meinhof to von Luschan, 18/10/09, 8/11/12.

racial term for a type viewed primarily as Caucasoid, has led to a racial theory in which the majority of the population of Negro Africa is considered to be the result of mixture between Hamites and Negroes. A prominent instance is the standard work of C.G. Seligman, *Races of Africa*, in which the Negroes of the West African forest belt become the only true Negro while all the rest are Hamiticized to a greater or lesser extent.<sup>102</sup> The speakers of “Nilo-Hamitic” languages are called racially half-Hamites. The Bantu are considered to be another type of Hamiticized Negro on the basis of speculations by Meinhof (for which he never produced proof, nor is any proof possible)...<sup>103</sup>

Greenberg thus considered Meinhof partially responsible for the confusion of language with race, which, he also contended, had led to the false categorizations of African groups.

Moreover, in the wake of Greenberg’s recasting of African linguistic classification, most of the old terms and groupings were cast aside and replaced with new categorizations and terms. Therefore, while Bantu languages are still viewed as a substantial to classificatory schemas, they are no longer seen as a separate family but, rather, ordered with the Niger-Congo branch, which also includes the Mande, Kwa, and Atlantic groups.<sup>104</sup> Meanwhile, the so-called “Hamitic” languages have been reclassified as Nilo-Saharan, and many of them renamed Cushitic. These groupings demonstrate that many of the relationships between languages that Meinhof and others of his generation posited were false, and in a sense based more on what he wanted to see, that is languages that corresponded with the “races” of their speakers.<sup>105</sup> This did not mean that post-war Africanist linguists were without prejudices of their own; as Edith Sanders pointed out in 1969, the newly identified “Cushitic” group had a suspicious correspondence with the “Hamites” of the past and had not been reordered in any significant way.<sup>106</sup> Still, on the whole the racist implications of Meinhof’s era have been erased from current scholarship.

While Meinhof claimed that his books were purely linguistic, then, they were in fact also anthropological and ethnographic, and Greenberg was correct about Meinhof’s overt emphasis on race. Although each of Meinhof and von Luschan’s three joint projects had a slightly different focus, all were preoccupied with the question of classification and how to categorize human beings based on both the languages that they spoke and on their physical attributes: the size and shape of their skulls, the color of their skins, the length and width of

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<sup>102</sup> Seligman 1930: 19, 213. Although Seligman’s work also goes beyond Meinhof’s categorization, describing groups – such as Nilotes and “half-Hamites” – that Meinhof himself may not have accepted.

<sup>103</sup> Greenberg 1966: 49.

<sup>104</sup> Williamson and Blench 2000: 12.

<sup>105</sup> Greenberg 1966: 24.

<sup>106</sup> Sanders 1969: 531.

their noses. Even today, without the racist rhetoric and dogma of the early twentieth century, there are still dangers as well as benefits associated with using linguistic, archaeological, and other kinds of evidence to reconstruct history. In his 1996 essay “New Linguistic Evidence and the Bantu Expansion” the historian Jan Vansina contended that much of the data linguists have used to unearth information on how different Bantu branches have come to settle where they are today is untenable. The accepted premise that the Bantu moved only in one direction as they traveled across the continent is, to Vansina, unacceptable, since it is more likely that they looped back and forth. Computational calculations of linguistic data have led to no fewer than seven possible routes that the Bantu could have taken as they colonized the continent, and there is no way to determine once and for all which path was the one that they actually followed.<sup>107</sup> Different kinds of data may seem to match on the surface and produce a uniform image of the past; when looking deeper, however, it might be discovered that information culled from different fields does not correlate as well as was first thought, or perhaps not at all. How much more complex the situation must have been around 1900, then, when disciplinary boundaries had not yet hardened, there was much less available data for scholars to work with, the issue of racial classification was a central preoccupation, and the colonial context largely defined the parameters within which Africanist scholars labored.

## **2. Meinhof, von Luschan; Linguistics, Anthropology**

What were some of the specific incongruities that surfaced in Meinhof and von Luschan’s joint work on Africa? How did these incongruities leave them open to attack on various fronts? In this section, I will address such questions by looking at the instances in which Meinhof and von Luschan used a combination of linguistic and anthropological material to make arguments about the nature of African culture, as well as by discussing a limited set of critical responses to their theories. Because Meinhof and von Luschan defended their collaborative practice even as they were cognizant of the methodological problems associated with it, I will also suggest some possible reasons for their continued insistence on examining race through a linguistic and anthropological lens.

Many of the difficulties that arose from Meinhof’s and von Luschan’s use of anthropological and linguistic data circled around their inability to reconcile classifications based on language with classifications based on race. Essentially, this meant that Meinhof and von Luschan struggled with how to define who was a “Hamite”, who was a “Bantu”, and who

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<sup>107</sup> Vansina 1995.



was a “Negrito”, or “Sudanic-speaking” African. Of all three groups, none were more problematic than the “Hamites.” Writing in 1929 and looking back over almost a century of research into the use of the word “Hamitic” in academic discourse, the Leipzig ethnologist Günther Spannaus maintained that “Hamites” could be described in at least three ways – in terms of linguistics, physical anthropology, and ethnology – and went on to complain that this was the main issue vexing scientists who grappled with how to categorize Africans:

one of the most interesting and important questions that had confronted African ethnology in the last decades is the so-called “Hamitic question”, i.e. the question of the origin of the Hamites and their significance for the cultural and social history of all Africa. The difficulties of the problem are grounded in the multiplicity of the concept “Hamite”, which shows different faces depending on whether you look at it from a linguistic, physical, or cultural perspective.<sup>108</sup>

Spannaus went on to explain that Hamitic languages were usually – but not always – the more sophisticated, inflecting languages spoken by lighter-skinned North Africans, whereas physical or “racial” Hamites comprised the “non-Negro element of the North African population”, and cultural Hamites were mostly cattle herders and raisers.<sup>109</sup>

In many ways, Spannaus summarized the parameters of a debate that had perplexed linguists and anthropologists alike from the end of the nineteenth century. The divisions among linguistic, physical anthropological, and ethnological “Hamites” that Spannaus discussed are, in fact, extremely evocative of Meinhof and Luschan’s semi-collaborative descriptions of Hamites and other African “racial” or “ethnic” categories earlier in the century.

Before turning to a discussion of the Hamites as they appeared in the work of Meinhof and von Luschan, though, it is important to understand the roots and antecedents of the theory that they espoused, which is commonly glossed as the “Hamitic hypothesis.” The idea of a “Hamitic” race or class of people has a history. During the Middle Ages all Africans were seen as Hamites, and were considered accursed because of their ancestor Ham’s failure to cover up the nakedness of his father, Noah. In the Bible, Noah punished Ham’s arrogance by condemning his descendants to be enslaved by the successors of his two brothers, Shem and Japhet, who had covered Noah up. Despite the fact that the Bible does not mention the skin color or outward appearance of Ham and his siblings, medieval interpretation held that Ham’s progeny – the Hamites – were African, whereas the children of Shem were the Middle Eastern Semites and those of Japhet the Indo-Europeans.

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<sup>108</sup> Spannaus 1929: 181.

<sup>109</sup> *ibid.*: 182, 186, 190-191.

With Napoleon's late eighteenth-century discovery that the "Hamitic" Egyptians were heirs to a complex and sophisticated cultures, however, the story shifted. In this retelling, only one branch of Ham's family, the Canaanites, had been cursed; the rest had developed the kind of civilization found in Egypt. Hamites were now redefined as the "Herren", or masters, of Africa, light-skinned and lordly.<sup>110</sup> This was the definition of "Hamite" that later scholars such as Meinhof, von Luschan, and Spannaus followed.

For Meinhof – as well as, it may be surmised, for von Luschan, Spannaus, and most of their contemporaries – the original biblical explanation of the Hamitic theory was of little importance to scholarly classifications of Africans. In Meinhof's version of the Hamitic hypothesis, the word "Hamite" denoted a scientific category, not a religious one. This may seem odd for a man who was both a pastor and maintained strong ties with the Protestant mission. Moreover, in his *Die Sprachen der Hamiten* (1912), the book that represented the culmination of Meinhof's thought on Hamitic languages and peoples, he claimed to accept the biblical interpretation that the children of Shem were Asian, those of Japhet Caucasian, and those of Ham African, at least on some level.<sup>111</sup>

The Hamites whom Meinhof discussed in his linguistic and cultural treatises were, though, still not the same as the Hamites of the Bible. Biblical Hamites were black, while linguistic ones were white.<sup>112</sup> Why would Meinhof have continued to apply a word with such a debatable meaning? Basically, he believed that, as the term "Hamitic" had already been largely accepted in scientific circles, it would not be possible to expunge it from scientific vocabulary. But how did he explain the fact that the word had come into such widespread use among academics? In *Die Sprachen der Hamiten*, Meinhof traced the popularity of the name back to what he perceived as a more general nineteenth century scholarly or philological prejudice:

If someone were to ask what the origins of this false terminology are, it is to be answered that, until recently, the educated world occupied itself almost exclusively with inflectional languages, thus essentially with the languages of the Caucasian race. Therefore this race, which was regarded as actual humanity, was divided into three large groups: the Indogermans (Japhet), the Semites and the Hamites. Today, when we know that most human languages are not included in this division, we can no longer be satisfied with it. The small admixture of Nigrific blood which the "Hamites" possess has

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<sup>110</sup> On the history of the Hamitic hypothesis see Sanders 1969; Spöttel 1996; Knappert 1975/76; Evans 1980; Drake 1959, among others. For its role in German academic thinking on Africa see also Rottland 1996.

<sup>111</sup> Meinhof 1912: VIII.

<sup>112</sup> Although as Evans (1980) demonstrates, at certain points biblical Hamites were indeed imagined as "white" and "red-haired."

therefore induced people to lump them together with the Negroes, from whom they are somatically and linguistically completely divergent.<sup>113</sup>

It is not easy to ascertain whether Meinhof was criticizing polygenist thinkers for having excluded the greater part of humanity from studies of language and culture, or philologists for not having recognized the complex number of forms languages could take.<sup>114</sup> Whatever the case, Meinhof was apparently uncomfortable with the term “Hamites”, a word that was in itself unstable and constantly subject to rapid redefinition.

This did not, of course, prevent Meinhof and von Luschan from using the word Hamitic – as well as other, related classificatory terms such as Bantu and Sudanic – in their three collaborative projects. The first of these, von Luschan’s article “On the Racial Affinities of the Hottentots”, which was coupled with Meinhof’s “The Language of the Hottentots,” attempted to answer the already well-worn question of whether the so-called “Hottentots” (Nama or Khoi) were racially and linguistically closer to the far-flung Hamites or the neighboring “Bushmen” (San). The second, von Luschan’s 1906 “Sechs Pygmäen aus Ituri” (Six Pygmies from Ituri) and Meinhof’s “Untersuchung der Pygmäensprachen” (Investigation of Pygmy Languages), did not address the “Hamitic” problem directly, but nonetheless took issue with the relationship between racial and linguistic categories, this time by exploring the makeup of the “Sudanic” “race”. Lastly, Meinhof’s 1912 *Die Sprachen der Hamiten*, to which von Luschan added a lengthy afterword, tried to put all the “pieces of the puzzle” together to assess how closely “Hamitic” languages and “races” were affiliated and, moreover, at what points they diverged. The book and essays also all highlight the fact that, while the Hamites were the most often discussed African “race” during the time that Meinhof and von Luschan were writing, in their minds the Hamites were also irrevocably connected to the other two. The “Hamitic hypothesis” as conceived of by Meinhof and von Luschan could not have existed without reference to other African populations.

In all three texts, Meinhof and von Luschan conflated their data and its results – which they had arrived at using very different methodological approaches – and saw their respective disciplines, African linguistics and physical anthropology, as *Hilfswissenschaften* that could assist each other in solving problems concerning African ethnicity and “race.” This was so even though von Luschan, for one, had originally rejected the entire notion of “Hamitenum” and believed – following the lead of the ethnologist Robert Hartmann – that all Africans

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<sup>113</sup> Meinhof 1912: VIII.

<sup>114</sup> As noted in Alter 1999, in the early nineteenth century Wilhelm von Humboldt had already discerned four general morphological categories: the inflectional, agglutinative, isolating, and polysynthetic. While it may have been true that more academic emphasis was placed on the inflectional languages than any other, then, Meinhof erred in suggesting that there had been little interest in non-inflectional languages.

shared an *Urverwandschaft* (original relationship). Early in his career von Luschan had maintained that there were, in effect, no differences among the Hamites, Bantu, and Sudanic speakers. Over the years, however, he claimed that the linguistic evidence had won him over, and by 1906, when he reported on a trip he had made to South Africa for the Berlin Society of Anthropology, Ethnology, and Prehistory, he declared that he was now convinced that language represented a “kind of index fossil, and one of our most important signposts in the still so murky terrain of African ethnology.”<sup>115</sup> Accordingly, von Luschan had become a firm and enthusiastic proponent of the Hamitic hypothesis, which held, in stark opposition to Hartmann or – a generation later - the French linguist Lilius Homburger (1940),<sup>116</sup> that Africa was made up of several different cultural and “racial” groups.

It was, moreover, Meinhof who presented von Luschan with the linguistic evidence that compelled him to alter his earlier stance. While Meinhof had achieved most of his academic recognition for the work he conducted on Bantu in the 1880s, he was always interested in examining a multiplicity of other African languages that, in his estimation, diverged quite dramatically from Bantu. This meant that Meinhof also wanted to investigate the “puzzle” of Hamitic languages already presented by such German scholars as Wilhelm Bleek, the first person to coin the word “Bantu” to describe a language group, the Viennese linguist Friedrich Müller, whose grammars divided the world according to the type of hair individual speakers had,<sup>117</sup> and Richard Lepsius, who had written a Nubian grammar and was widely known for having created a standard system of orthography for the transcription of languages not written in roman scripts.<sup>118</sup> Meinhof hoped to extend their research and, in so doing, finally determine the answers to such questions as how to position the northwest African Fulbe, whose language he posited at a developmental stage between Bantu and Hamitic, or where the East African Maasai had first originated and the extent to which they might be “Semites”.

Although Meinhof and von Luschan were good friends who enjoyed several rich intellectual exchanges, Meinhof at times insisted that anthropological classifications could not, in the end, bring anything to bear on linguistic ones. He strove to avoid some of the traps

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<sup>115</sup> Von Luschan 1906a.

<sup>116</sup> For more on Hartmann’s ethnological philosophy as it related to Africa see Ciz 1984: 71-74.

<sup>117</sup> Not to be confused with the philologist Friedrich *Max* Müller, who taught for many years in England.

<sup>118</sup> See Lepsius 1855 and 1880.

into which he believed various of his predecessors had fallen, and maintained that his analyses of African relationships were based purely on linguistic – not anthropological or “racial” – evidence. Meinhof was, for example, extremely skeptical of Müller’s linguistic categories, considering them to be grounded more in anthropological than linguistic criteria. In his multi-volume *Grundriss der Sprachwissenschaft* (Outline of Linguistics), Müller had indeed classified African languages according to hair type, devoting one volume to the languages of straight-haired peoples, one to the languages of curly-haired peoples, and one to the languages of “woolly-haired” peoples.<sup>119</sup> Meinhof thus negated Müller’s proposed classificatory schema at the 1905 German Colonial Congress, where he held that “linguists can receive much stimulation from physical and cultural anthropology and vice versa – but there is no doubt that there must be a methodological error when anthropological points of view are prematurely approached in linguistic investigations.”<sup>120</sup> As a possible correction to Müller, Meinhof suggested that purely linguistic phenomena – such as the existence of grammatical gender in noun classification, inflection, or the appearance of clicks – be compared to determine whether or not two languages were related, and that physical traits should not be examined, at least not immediately.

Yet undergirding Meinhof’s work – not to mention von Luschan’s – was still the belief that “Hamites” were in some way originally “white.” Through intense racial mixing over hundreds of years, some, like the “Hottentots” or Nama, might have taken on the physical characteristics of the neighboring “Bushmen”. This did not, however, detract from their original position as racial “Hamites.” As Luschan explained in “On the Racial Affinities of the Hottentots” –originally presented as a paper for the South African Association for the Advancement of Science in 1905 – whenever two groups met and intermingled, their languages and “somatic” types were never equally influenced. Von Luschan maintained: “As a matter of fact, we see the somatic type depending on the numeric relation between the old native population and the newcomers.”<sup>121</sup> This effectively meant that members of an “attacking” tribe would eventually lose most of their distinguishing physical characteristics. They would probably have brought few women on their travels and, as a result, have married the women they conquered. Even so, the fact that they had once been “Hamites” would never be completely obscured, as Luschan also asserted that

we see the mental culture develop itself independently of all numeric relation, and the superior language, the superior religion, and (if there is any) the superior writing, survive. In a few cases we might see the old language survive with women and children;

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<sup>119</sup> Knappert 1975/76: 308, after Müller 1876-88.

<sup>120</sup> Meinhof 1905b: 119.

<sup>121</sup> Luschan 1905: 115.

but generally the language, or at least the grammar, of the superior invaders survives [sic], and very often is actually adopted by the native race.<sup>122</sup>

There was, then, an underlying notion that “tribes” whose languages possessed a certain morphology and grammar, and usually exhibited grammatical gender, were very possibly descended from “racially pure” Hamites.

While von Luschan had begun the same paper with the underlying assumption that all sub-Saharan African languages could be parceled out into the Hamitic, Sudanic, or Bantu categories except the “the Arabs, the Portuguese, the Boers, and other people [who have] immigrated recently... (and) the Bushmen, the Hottentots, and the various pigmy [sic] tribes of tropical Africa”,<sup>123</sup> he quickly moved towards “proving” Hottentot affiliation with the Hamites. Von Luschan was able to do so because Meinhof bolstered his findings with his own article, “The Language of the Hottentots”, which von Luschan had brought with him to South Africa. Here, Meinhof rounded out von Luschan’s argument with technical details, asserting that because Hottentot languages exhibited grammatical gender – i.e. the division of nouns into separate classes according to whether they were male or female – they had to be grouped among the inflecting languages, the only ones that Meinhof believed showed this trait. The only African languages that were considered inflecting at the time were those of the Semites and Hamites. Meinhof did not question whether the Hottentots were Semites, but concluded that, on the basis of the type of language they spoke, they had to be Hamites.<sup>124</sup> This furthermore indicated that their languages were somehow related to those of Indo-Europeans, which were also inflecting. The implication here was that, far from being a “prehistoric” people, the Hottentots were in fact connected to the highly sophisticated and advanced Indo-Europeans. As von Luschan had suggested, they had lost their “racial” attributes as they mixed with the Bushmen, but their “superior” language still remained.

Linguistic arguments also supplemented anthropological ones in Meinhof’s and von Luschan’s discussion of a group of Central African Ituri “pygmies” who visited Germany as part of a *Völkerschau*, or people exhibition, in 1906. Once they were in Berlin, both von Luschan and Meinhof submitted them to a battery of tests to determine their origin and their degree of relation to other African “races”, including the Bushmen. In other words, Luschan and Meinhof tried to categorize the pygmies on both linguistic and physical grounds. Luschan, for example, reported that the pygmies’ language was one of the factors that had convinced him they were truly from the rainforest region, and thus “authentic” pygmies, not

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<sup>122</sup> Ibid.

<sup>123</sup> Ibid.: 111.

<sup>124</sup> Meinhof 1905: 121.

the “fakes” which had been paraded around Europe in the past.<sup>125</sup> Luschan made this claim despite his own admission that information on pygmy languages – not excluding that which he himself had gleaned from listening to and speaking with the Ituri group – was scant at best, and difficult to pin down. Thus, while Luschan only mentions “Hamites” in passing – he suspected that one of the pygmy women “perhaps had some Hamitic blood”, as she was lighter-skinned than her companions and had a “strikingly small nose”<sup>126</sup> – his effort to classify the pygmies still constituted an attempt to “order” a piece of the African racial and linguistic map according to the “organizing principle” of the Hamitic hypothesis, which dictated that Africans could be identified by certain, specific grammatical and biological traits.

Where Luschan’s remarks on pygmy language were introductory and probably designed to make his audience aware of the important relationship between anthropology and “colonial” linguistics, in his part of the essay Meinhof turned to a more complex and detailed analysis of pygmy speech. He noted that the so-called “pygmy” language was similar to the West African languages that, on the basis of their isolating structures, had recently been classified as “Sudanic.” In sharp contrast to the inflecting Hamitic and Indo-European languages, Sudanic languages were considered simple in terms of grammatical structure, made up of strings of roots that were “isolated” from each other and could not be combined to create new words and meanings. Like Ewe, which was widely spoken in the German colony of Togo, Meinhof held that Pygmy was isolating, and that both languages, Ewe and Pygmy, also made “rich use” of musical tone. The pygmy language, he admitted, had obviously adopted much Bantu vocabulary, so that the vocabulary for the numbers one to five, for instance, were all of Bantu origin. This did not, however, detract from the language’s obvious “Sudanic” structure, one which “Bushman” also shared. Meinhof did not say whether he assumed a genetic relationship between the pygmies and the Bushmen. The pygmies’ language had no clicks, which were a fundamental element of the Bushmen’s. Yet they did contain velar-labials (“sounds which are produced by a narrowing of the soft palate and a simultaneous occlusion of the lips”<sup>127</sup>) which, Meinhof surmised, were possibly either related to clicks, or could develop into them.<sup>128</sup>

A combination of anthropological and linguistic information was thus mustered to support von Luschan’s and Meinhof’s conclusions that the pygmies were both Sudanic-

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<sup>125</sup> Von Luschan 1906b: 718-9.

<sup>126</sup> Ibid.: 725.

<sup>127</sup> Ibid.

<sup>128</sup> Ibid.: 731.

speakers and probably related to the Bushmen. The correlation between anthropological and linguistic data also seemed to move them a step closer to “proving” pygmy primitivity, as it allowed them to fit the pygmies into a certain developmental slot. That their collaborative methodology raised certain overarching problems, however, became apparent in their final joint enterprise, *Die Sprachen der Hamiten*. Most of the book was devoted specifically to solving linguistic questions; in it, Meinhof analyzed the languages of a variety of African groups from across the continent in order to ascertain their position as Hamitic. Among the peoples studied were the Hausa, Maasai, Somali, and Fulbe. Not all of these languages were, it should be noted, universally considered part of the Hamitic language “family”; for example, the year before *Die Sprachen der Hamiten* was published Meinhof wrote an entire article about Fulbe, positing it as a *pre-hamitic* language on the cusp of blossoming into a Hamitic one.<sup>129</sup> In the book, though, Fulbe was included among the Hamitic languages and - whatever Meinhof’s protestations that his analyses were not racial or cultural - evidently also within the Hamitic “race”; he referred to them as a “herding and ruling people among the Negroes” which travelers had described as “proud, manly, and warlike.”<sup>130</sup>

The Fulbe – like the neighboring Hausa – were a classic instance of a group where “race and language” did not match and presented scholars such as Meinhof with a predicament. Physically, the Fulbe were considered “light-skinned Hamites”, but nonetheless they spoke a language that in many ways seemed closer to that of the darker-skinned Bantu. Meinhof explained this surprising, confusing circumstance by maintaining that a nomadic group speaking a language similar to that of the Fulbe had conquered a pastoral, “Nigritic” people, and that this resulted in a “mixed” ethnicity that was neither entirely Hamitic nor entirely Sudanic. Meinhof also argued that it was the very proximity to true “Negroes” of groups like the Fulbe and Masai that made them so hard to identify as Hamites and led some to classify them as Sudanic.<sup>131</sup>

The Fulbe were not the only group to raise problems for Meinhof’s and von Luschan’s classificatory scheme. The Maasai of East Africa also emerged as somewhat difficult to order. In 1904, Meinhof wrote a review of a book on the Maasai (*Die Masai*) written by Moritz Merker, an officer in Germany’s colonial army. Throughout the book, drawing on linguistic, anthropological, and quasi-historical information, Merker maintained that the Maasai were in fact not originally African but Semitic, and very likely related to the Arabs of the Middle

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<sup>129</sup> Meinhof 1911.

<sup>130</sup> Meinhof 1912: 31.

<sup>131</sup> *Ibid.*: 2-4.



East.<sup>132</sup> While Merker did not have any specific linguistic training or background, he clearly suggested that the Maasai language was Semitic.

In his critique, Meinhof attacked and dismantled Merker's contention that Maasai was Semitic. Instead, he questioned Merker's understanding of Maasai and the importance he had placed on the language in his analysis. Meinhof asserted:

Even if the author does not want to give linguistics the first word in deciding this question, we will, if we have to talk about Semites at all, not be able to dispense with it. The label "Semitic" is in scientific circles first applied to linguistic observation. The Phoenicians are counted as "Semites" because they speak a Semitic language, even though biblical reports place them among the Hamites; Arabs are called "Semites" for similar reasons, even though "Hamitic" admixture has here been maintained since ancient times. Jews are also considered Semites, despite their, as the author knows, strong mixing with alarodic [sic] blood. If one wants to use the term "Semites" in this sense, then, the Maasai do not belong, as one does not require much time studying their language to see that they are not, in this sense, Semitic. Linguistically the Maasai are Hamites and there can seriously be no question of their belonging to the Semites.<sup>133</sup>

Here Meinhof challenged Merker on the grounds that his linguistic analysis was not precise, and that this had led him, perhaps through racist observations, to categorize them incorrectly.

In his discussion of Merker, Meinhof thus points to a serious rupture between linguistic and anthropological classifications of various ethnic or "racial" groups. Linguistic and anthropological Hamites – and hence Sudanic and Bantu speakers – did not correlate on a one-to-one basis. The Phoenicians were not racially Semitic, even if they spoke a Semitic language; the same held true for some Arabs. Further, while Meinhof completely rejected Merker's assertion that the Maasai language was Semitic, he was evidently not so sure about their "race" or "culture". For Meinhof, Merker's problem rather rested in his inability to disentangle ethnological, anthropological, and linguistic data. He was able to find some merit in Merker's argument, insofar as there was little doubt in his mind that Hamites and Semites were ethnologically and anthropologically related and perhaps shared an *Urverwandschaft* (originary relationship).<sup>134</sup> They also had some linguistic traits in common, as both the Hamitic and Semitic language families were inflecting and exhibited grammatical gender. Moreover, there were plenty of Semitic loan words in Hamitic languages, not to mention Hamitic loan words in Semitic languages.<sup>135</sup>

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<sup>132</sup> Merker 1904.

<sup>133</sup> Meinhof 1904: 738.

<sup>134</sup> Ibid.: 740.

<sup>135</sup> Ibid.: 739-740.

The Rwandan Hima, Tutsi, and Hutu were also difficult to categorize. In the afterword to Meinhof's *Die Sprachen der Hamiten*, which was titled "Hamitische Typen", von Luschan addressed the issue of "typical" Hamitic biological and racial features. Unsurprisingly, these included light skin, small noses, and tall, thin physiques. Even as Luschan maintained in the afterword that it was often the dissimilar pair of "negroid" body type and "Hamitic" language that prevailed after culture contact, the Hima and Tutsi obviously presented him with a dilemma. They were, he believed, far more "white" than the Maasai, and loath to mate with their Bantu "subjects" (*Untertanen*). Still, and puzzlingly, they had evidently assumed the latter's language. "We currently have no satisfactory explanation for this actually very surprising deviation from the norm", Luschan mused,

and it is not yet out of the question that, in the case of individual Hima tribes, we might still succeed in discovering the remnants of their own old languages." At the moment, though, it appeared as if the phantom languages had permanently evaporated and "sunk into the ocean of the forgotten."<sup>136</sup>

However, Meinhof and von Luschan also glided over this caesura, ultimately not giving it primary importance or allowing it to detract from their shared conviction that Hamites were "white" rulers and Sudanic "blacks" their underlings.

The inclination to conflate language with race – and linguistic data with physical evidence – was therefore strong in publications such as Meinhof's and Luschan's. Even during their own time there were, moreover, those who criticized and decried such conflation. Shortly after the 1912 publication of *The Languages of the Hamites*, Hugo Schuchardt, a prominent and sometimes controversial linguist from the Austrian university of Graz, criticized Meinhof for failing to recognize the difference between linguistic and racial phenomena. For Schuchardt, who had written a review of *The Languages of the Hamites*, racist thinking permeated the book – which, it will be remembered, Meinhof claimed was based entirely on linguistic principles – and condemned both Meinhof and von Luschan to remain caught in a trap of circular logic:

Both the questions "What are Hamitic languages" and "What are Hamites" are clearly dependent on one another, and the answer must either be "Hamitic languages are those that are spoken by Hamites" or "Hamites are those who speak Hamitic languages." The one is an anthropological explanation, the other a linguistic. Meinhof would admit to the wording of the first when he says "languages of the Hamites" instead of "Hamitic languages"; but in fact, here he can only want to express one wish, that of seeing the tribes which speak Hamitic languages as a unified group not only in language but also

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<sup>136</sup> Von Luschan 1912: 251. While in this instance von Luschan did not grapple with Meinhof's hypothesis that the Bantu were perhaps the product of a centuries-long intermingling between white Hamites and black speakers of Sudanic languages, he did allow the linguist to insert a footnote to this effect.

in physical constitution. To this end, he reaches his hand out to his anthropologist friend Felix von Luschan; but he decidedly supports himself on (Meinhof's) shoulder, and cannot escape the linguistic explanation of "Hamites", and thus we go around in circles.<sup>137</sup>

Elsewhere, the American anthropologist Edward Sapir similarly challenged the book's use of anthropological and linguistic definitions.<sup>138</sup> Finally, at the German Colonial Congress of 1910, the amateur anthropologist and entomologist Paul Staudinger verbally chastized Meinhof for a speech he had held on the current situation of African linguistic research:

With all the previous research and knowledge, we can speak of a Hamitic language group. However, to classify all of the different people who speak a Hamitic idiom as somatically and anthropologically the same – as "Hamites", and thus as a "race" – is simply not possible.<sup>139</sup>

It is interesting to note that Meinhof did not directly answer Staudinger's criticism; instead, he evaded it by stating that "the difference between anthropological and linguistic modes of observation is obvious, but does not hinder the mutual support of these sciences."<sup>140</sup> In this case, the fact that the fusion of anthropological and linguistic classificatory criteria raised a number counter-arguments was for Meinhof outweighed by the perceived benefits of interdisciplinary methodology. Meinhof admitted that knowledge of a group's language would not allow scholars to determine its race conclusively – there had been too much "intermingling" for that. However, he still felt it was possible to speak of Hamitic *types*, which was why he had asked von Luschan to write his afterword on their physical attributes.

Even so, while Meinhof essentially glossed over Staudinger's comment at the Colonial Congress, both he and von Luschan were quite aware of the difficulties that could arise if they used both anthropological and linguistic criteria as bases for human classifications. Meinhof's inability to classify groups such as the Fulbe and Maasai in itself testifies to a certain amount of confusion. Further, the issue of whether – and to what extent – racial typologies factored into linguistic ones, and vice versa, was also a subject that emerged in their correspondence. In a letter to von Luschan dated 28 November 1907 Meinhof commented that, however much people might complain about how von Luschan "mixed up" or "confused" linguistics and anthropology, he believed that his colleague was fully cognizant of the differences between the fields. Indeed, he added, von Luschan "wanted to know the disciplines separately" but, at the same time, did not simply want to "exclude the possibility that language is at least a very

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<sup>137</sup> Schuchardt 1912.

<sup>138</sup> Sapir 1913.

<sup>139</sup> Staudinger 1910.

<sup>140</sup> Meinhof 1905b: 106.

important ethnographic feature.”<sup>141</sup> Meinhof never went so far as to state that language and race had a one-to-one correspondence, however, explaining in another communication that the Bantu – whom Meinhof ultimately described as a “mixture of Hamitic and Nigritic” with very variegated “somatic” qualities – proved that they were two distinct subjects.<sup>142</sup>

Meinhof and von Luschan were, moreover, not the only ones who recognized the fragility of the border between their young disciplines. Otto Dempwolff, a medical doctor who had received language training at the Seminar for Oriental Languages in Berlin, also strove to keep linguistic and anthropological criteria apart. In 1916 Dempwolff, then a colleague of Meinhof’s at the Hamburg Colonial Institute, published a book on the Sandawe of East Africa, which was based on field research he had conducted in the area. While Dempwolff asserted that the Sandawe language – which contained “clicks” similar to those found in “Bushman”, “Hottentot”, or certain South African versions of Bantu – was Hamitic, he was unwilling to make any conclusions about Sandawe ethnicity or “race.” Instead, he emphasized that

this hypothesis is valid only for linguistics: it should not in any way anticipate anthropological investigations or conclusions, and likewise has nothing to do with the ethnological makeup of the Sandawe<sup>143</sup>

In this sense, Dempwolff was perhaps *more* careful than Meinhof, as he explicitly designed a book that would focus on Sandawe language and culture separately.

Meinhof – and probably also von Luschan – were thus conscious of the problems that their research methodologies might expose. However, at least for Meinhof, the benefits of combining information from linguistics, ethnology, and physical anthropology far outweighed the risks. Meinhof was personally consumed with producing a linguistic and ethnological “map” of Africa, in which he would be able to identify the origins and subsequent movements of every “tribe.” The project would help clarify the “murky” ethnic situation in Africa, and make the continent’s culture that much more accessible to German colonists. Meinhof was, then, aware of the pitfalls of conflating evidence, but nonetheless willing to use anthropological data to bolster his own linguistic findings, especially when there were gaps.

### 3. Some Consequences of Conflation

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<sup>141</sup> Staatsbibliothek zu Berlin, Nachlass Felix von Luschan, Meinhof to von Luschan, 28/11/07.

<sup>142</sup> Meinhof 1910c: 164.

<sup>143</sup> Dempwolff 1916: 69-70.

There are not necessarily any direct links among the tensions that existed between the theoretical precepts of African linguistics and anthropology and the institutional development of the former in the late nineteenth and early twentieth centuries. However, it is also evident that, after the establishment of the Berlin Seminar for Oriental Languages in 1887 and the Hamburg Colonial Institute in 1908, there was an increased call for “professional” African linguists. At first, the colonial government had been satisfied with the missionaries and civil servants who, upon returning from the field, took assignments as language teachers in the metropole. However, as the “colonial sciences” became more generally entrenched in Germany, representatives of both the Berlin Seminar and Hamburg Institute recognized the need for more “academically inclined” instructors, often those with university training in philology. As the field itself was increasingly defined by scholars such as Carl Meinhof and Diedrich Westermann, assuming its own journals and gaining international renown, there was a concurrent move toward the solidification of disciplinary “schools”, in which younger scholars were trained by their elders.

So it came to be that in 1930, when the University of Leipzig was searching for a scholar to replace its outgoing professor Hans Stumme and eventually take control of its Institute of African Languages, two of the four contenders for the position were dismissed out of hand - one of them because he had too little ethnological background, and the other, ironically, because he had too much. All the candidates had some training in both “Bantu” and “Hamitic” languages, which was, it would seem, crucial for the job. According to the Dean of the Philosophical Faculty, the University recognized that “Africa’s linguistic world is quite extensive and varied” and that “Semitic, Hamitic, and Bantu languages have to be equally considered.”<sup>144</sup> From the correspondence, however, it becomes clear that the faculty required something more: that someone have actual “field” experience was considered a marked advantage, as was the person’s academic “designation” or general recognition among his peers as a pedigreed linguist or philologist. This meant that Martin Heepe – who had worked with Meinhof but also criticized him – was excluded on the grounds that he

work(ed) in a purely philological realm, without consideration of the circumstances or the realization that language is not a self-sufficient life-form but a part of the entire culture and, consequently, can also only be understood as such.<sup>145</sup>

In the meantime, Bernhard Struck – who was not even on the original list of candidates suggested for the position and was proposed by a secondary committee – was rejected for the

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<sup>144</sup> StaH, Hochschulwesen Dozenten u. Personalakten IV, 1177. Ergangen 30, Ministerium für Volksbildung, Sachbetreff Dr. phil. August Klingenheben, d. 26/1/67, Gotz, Dean of the Philosophical Faculty, Leipzig, to Ministry of Education, Dresden, 12 March 1930.

<sup>145</sup> Ibid., Report "Betr. die planmäßige außerordentliche Professur für Neu-Arabisch und hamitische Sprachen Afrikas an d. Universität Leipzig", 15 March 1930.

completely opposite reason that he was, even by his own previous admission, far more an ethnologist and physical anthropologist than a linguist.<sup>146</sup>

Both rejections were probably also motivated by personal factors: neither Heepe nor Struck were popular among their colleagues, and Heepe was considered particularly abrasive. This may, indeed, have been the reason behind his characterization as not ethnologically-minded, since his supporters pointed out that he had more field experience than August Klingenberg, the University's top candidate and eventual choice, and that some of his detractors had quarreled with him precisely over anthropological, and not linguistic, matters. Struck's case, however, is more difficult to unravel, and is tied more directly to the intellectual and theoretical issues outlined above, relating to the "Hamitic" concept. While his proponents – ostensibly several unnamed members of "related" disciplines including ethnology, geography, and colonial agriculture, who wrote a letter extolling him to the Philosophical Faculty – considered Struck especially well-suited for the position because his "ethnological works concern themselves exclusively with problems that appear important for the solution of linguistic problems", his detractors were just as convinced that he was more of a dilettante than anything else. This was not entirely untrue, as Struck had studied ethnology, physical anthropology, geography, and linguistics, and worked in the museums of *Völkerkunde* in Berlin and Dresden.

Perhaps even more poignant, however, was the fact that, in his linguistic publications, Struck often drew concrete connections between the questions of "race" and language that had divided both linguists and anthropologists. Struck's 1921 "*Somatische Typen und Sprachgruppen in Kordofan*", for instance, was culled from linguistic evidence that Meinhof had collected during a 1914 trip to the Anglo-Egyptian Sudan, but went far beyond it in linking biological traits, including skin color and head size, to Meinhof's data.<sup>147</sup> Meinhof himself was, indeed, more befuddled by the article than anything else; after Struck had sent it to him for perusal, Meinhof thanked him briefly but admitted that he did not understand the bulk of it.<sup>148</sup> In rejecting the proposal that Struck be considered as an "Orientalist" or "philologist", Leipzig's Philosophical Faculty obliquely mentioned the essay, as well as the entire "Hamitic" debate and Struck's position in it:

In ignorance of the incorrectness of the idea that there is a direct connection between language and race, which has long been the recognized position and scholarly

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<sup>146</sup> Ibid., Philosophical Faculty, University of Leipzig, to Ministry of Education, Dresden, 1 April 1930.

<sup>147</sup> Struck 1920/21.

<sup>148</sup> Archives of the Institut für Afrikanistik und Äthiopistik, Hamburg, Ethnogr.-anth. Briefwechsel BV 2 (b) vom 3 Oktober 1920 bis 1. September 1932, Carl Meinhof, Hamburg, to Bernhard Struck, Dresden, 28 May 1921.

standpoint of linguistics, he searches to unite ambiguous calculations of the mathematical relations of skull bones to morphology.<sup>149</sup>

It was, then, Struck's insistence on equating racial and linguistic evidence that, in the end, forestalled and eventually thwarted his candidacy; for he, even more so than a Meinhof or a von Luschan, was unable to avoid the trap of conflation.

We have seen that as the discipline of African linguistics – *Afrikanistik* – was first forming, it was highly fluid, and its practitioners conceived of their science as one that would, in conjunction with physical and cultural anthropology, answer manifold difficult questions about Africans. As time went on, however, it became increasingly evident that linguistics and anthropology were separate domains that did not always produce information that dovetailed with each other's results. Moreover, with progressive institutionalization, the boundaries between *Afrikanistik* and other "colonial sciences" hardened, perhaps to some extent in recognition of their irrevocable differences. Whether the later stiffening of boundaries also had something to do with the end of German colonialism, I am not sure. Even in the 1920s and 1930s *Afrikanistik* was seen as an "auxiliary science" to anthropology; however, the question requires further examination.

#### 4. Conclusion

Let us return briefly to Czekanowski and the intellectual milieu that he entered when he arrived in Berlin at the start of the twentieth century. Czekanowski may not have had much to do directly with Meinhof, but through his dealings with von Luschan, he may have been affected by the debates over the application of linguistic and anthropological information to solve questions about the classification of Africans, as well as finding himself caught up in the disciplinary shifts that were occurring in both linguistics and anthropology at the turn of the twentieth century. While the example of von Luschan and Meinhof is specific, it also had wider ramifications for the more general development of African studies in Imperial and Weimar Germany, if not beyond. During this period, the institutional structures for Africanist linguistics, physical anthropology, ethnology, and related fields were still in a very nascent stage, and there were very few people who could claim to be "professionals" in any one area. In the case of African linguistics, practitioners came from a wide range of backgrounds, but very few had formally studied African languages or cultures; indeed, they could hardly have done so, since programs for such studies had only just begun to emerge. Disciplines which lacked experts therefore culled knowledge from a variety of sources in the hope that this would provide greater understanding of a region about which Germans still understood

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<sup>149</sup> Philosophical Faculty, Leipzig, to Ministry of Education, Dresden, 1 April 1930.

relatively little. Linguists such as Meinhof may not necessarily have wanted to make race a central theme of their work – as can be seen from his struggle to separate it from language – but race always returned, in part as a function of the need for disciplines to be *Hilfswissenschaften*, in part because it was a prominent issue throughout society, far beyond the African sphere. And this was the context into which Czekanowski stepped when he left Poland for Germany.

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## **Czekanowski and the National Socialist Administration**

*Katja Geisenhainer and Udo Mischek*

This article deals with Czekanowski's ideas regarding physical anthropology and the reaction of German anthropologists to them. Special attention will be given to his role in relations between Polish and German anthropology during the 1930s. Czekanowski's theories caused serious problems for scientists in Nazi Germany and for those bureaucrats engaged in justifying German expansion in Eastern Europe. In particular the Upper Silesia problem was much debated.

### **1. The role of physical anthropology in the interwar period**

When Poland regained its sovereignty in 1918, German fears of further Polish territorial claims increased. There followed a call for a well-directed *Volkstumsarbeit*, which meant to promote national consciousness in varied ways and to suppress every anti-German movement.

Many physical anthropologists felt the need to support such *Volkstumsarbeit*. They endeavored to draw up a common model of identification by developing a view of history that covered thousands of years. This was by no means a new task for anthropologists. Especially since the second half of the 19<sup>th</sup> century there was very close co-operation between the disciplines of physical anthropology, ethnology and prehistory until the middle of the 20<sup>th</sup> century, not least because of this very effort to write a comprehensive history of mankind. But with the growth of nationalism it became more important to reconstruct the past of one's own nation or of one's own people. Frequently, this reconstruction was connected with the intention to legitimate political, geographical and cultural rights.

The main interest of the physical anthropologists was in race. An important component of their conception was the indivisibility of race and culture. For many anthropologists "race" was a clearly defined constant which could only be changed through mixture with other races. On the basis of this theory, linked with the methods of archeologists and prehistorians, newly



discovered skulls and skeletons were analyzed. Often these analyses were connected with value judgments. In particular the classification of long [*dolichocephale*] and broad [*brachycephale*] skulls – the "longformed" skull was rated higher – included a lot of physical, psychological and character attributes.

If skulls with different forms were found in one place, it was assumed that two or more races had existed there at the same time – a situation which was explained by immigration of at least one of the groups. A variability of skulls, which indeed exists even among brothers and sisters, was denied with regard to races. Starting from the conception of race as a pure and constant entity, anthropologists, historians and others not only propagated an ideal typical skull for each race, but also constructed different theories of migration and displacement in order to explain similar findings in different places. These explanations were rarely free of ideology. They tried to explain current conflicts and problems from the past and argued that these conflicts corresponded to the laws of nature.<sup>150</sup> In this way they envisaged a biologically constructed steadiness, a racial and cultural continuity over thousands of years. Based on this myth of unity, purity and continuity the revision of the German borders was claimed as a natural right.

Not only German anthropologists but also their Polish counterparts researched racial stock in this way. However, it was not generally accepted that particular racial categories could be equated with their supposed national counterparts – as for example in *nordisch* [Nordic], = German and *sudetisch-präslawisch* [Sudetic-Pre-Slavic], *ostisch* [eastern], *osteuropid* [East European], *ostbaltisch* [East Baltic], = Polish. Also in the opinion of some Polish anthropologists the people in Poland belonged to the *Nordic* race – even more so than the Germans. Everybody reproached their opponents for being unscientific and for merely supporting political interests. Meanwhile there were different views as to which criteria might be used to determine race and whether the phenotypes, genotypes or psychological characteristics were more important. Accordingly there was little consensus about how to assign a person to a race. Researchers involved in this discourse strove to establish their own approach by discrediting other theories and methods. With the year 1933 this discussion gained a new urgency.

While German race researchers were analyzing different groups of the population anthropologically, discussing different approaches with colleagues and with members of the Nazi Party, publishing or planning to publish their results, the German government had to ensure that the German people remained an inseparable, integrated whole and could feel that too. New organizations were set up in which scientists of different disciplines came together.

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<sup>150</sup> Cf. for example Fetten 1991: 10 f.

With the aim of revision of the borders they discussed the possibilities of eastward geographical, political and economic expansion. Their papers and findings were of great importance for government officials who had to deal with the provinces in the East.

In 1933 the historian Albert Brackmann organized a conference at which the "North and East German Research Community" (NODFG) was founded. Its aim was to coordinate all research concerning the East. Brackmann explained:

Due to the strong and threatening activity of Slav scientists, the goal absolutely needs to be the powerful concentration of all scientific forces involved in the *Deutschtumsarbeit* of the Northeast for fruitful and unified research. There have been no more doubts about realizing this idea, which has existed before and has just recently been supported massively from different sides since the *Führer* Adolf Hitler gave the guidelines for a clear and goal-oriented Eastern policy of the New Germany.<sup>151</sup>

Brackmann had founded the *Publikationsstelle* (PuSte) in 1929 and was its chairman until 1936. This institution, located in the *Preussisches Geheimes Staatsarchiv*, served as a link between the scientists and the authorities responsible for political questions concerning Eastern Europe. The PuSte was a research center whose main purpose was observation: Acting upon orders from the Foreign Office and the Ministry of the Interior, the PuSte had to evaluate the "anti-German Polish newspapers": "We have supplied about 250 ministerial, state, party, military and scientific authorities with our Polish press excerpts. The highest authorities of the Reich, party and military ask constantly for information."<sup>152</sup> One of the Slav scientists whose activities were monitored was Jan Czekanowski.

## 2. Czekanowski's theoretical position

Czekanowski had studied mathematics, anatomy and physical anthropology and had written his dissertation on *Untersuchungen über das Verhältnis der Kopfmaße zu den Schädelmaßen* ("Studies on the Relation of the Size of the Head to that of the Skull") in 1907. He pursued his

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<sup>151</sup> 19.12.1933; BArch, R153/1269. "Das Ziel ist die angesichts der überaus regen, bedrohlichen Aktivität der slawischen Wissenschaftler dringend gebotene stosskräftige Zusammenfassung aller in der *Deutschtumsarbeit* des Nordostens stehenden Kräfte der Wissenschaft zu einheitlich ausgerichteter, wirklich fruchtbarer Tätigkeit. Der Verwirklichung dieses an sich schon alten und gerade neuerdings wieder von vielen Seiten nachdrücklich vertretenen Gedankens stehen, seit dem *Führer* Adolf Hitler die Richtlinien einer klaren, zielbewußten Ostpolitik des neuen Deutschlands vorgezeichnet hat, keine Bedenken mehr entgegen."

<sup>152</sup> PuSte to Geheime Staatspolizei II P, Berlin, 12.10.1939; BArch, R 153/665. "Mit den von uns hergestellten ‚*Polnischen Presseauszügen*‘ wurden ca. 250 ministerielle, staatliche, parteiamtliche, militärische und wissenschaftliche Stellen beliefert. Aus demselben Stoff baut sich grossenteils unsere Auskunftstätigkeit auf, zu der wir laufend von den höchsten Reichs-, Partei- und Heeresstellen herangezogen werden."

research in physical anthropology later in his life. There was little difference between his ethnological and anthropological orientations: his main concern was to find laws in both the physical and the social sphere of human life. His 1911 paper *Objektive Kriterien in der Ethnologie* ("Objective Criteria in Ethnology") clearly shows his perception of ethnology: Czekanowski was convinced that using his method of dealing with what he called "*Assoziationskoeffizienten*" would lead to a much greater exactness in ethnology.<sup>153</sup> His works in physical anthropology follow the same path: Czekanowski was searching for the exact method in mathematical formulas.

In one of his earlier papers concerning Polish anthropology, written in 1911, Czekanowski stated that the consequences of older anthropological research in western Europe did not fit with the reality he found in Slavonic countries. Criticizing the older model that accepted only three racial types (Nordic, Mediterranean and Alpine), Czekanowski argued for a model that allowed for a much greater variability of physical types. He opted for a model of four main types with the possibility of combinations among these types. In Czekanowski's early papers he was mainly concerned with the "Pre-Slavic type", whereas his later publications dealt more prominently with what he called the "Nordic type" in Poland.<sup>154</sup>

Czekanowski continued to work with his four-race-model at least until 1939. To determine the proportion of the four racial elements and their mixture was his main concern. The result of his investigation was to introduce ten "anthropological formations" which he conceived of as mixtures of the four main races.<sup>155</sup> On that score he worked in accordance with most of his German colleagues.<sup>156</sup> In this article as well as in his popular writings Czekanowski saw Poland in his time as a country dominated by the "Nordic" elements.<sup>157</sup> Furthermore, it was in Czekanowski's eyes a reservoir of the Nordic element. In contrast to his German colleagues, who feared a "denordisation" of their country, Czekanowski ascertained a surplus of Nordics in

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<sup>153</sup> Czekanowski 1911a: 75. Here he wrote: "*Aus diesem Grunde muß man die Methode der Assoziationskoeffizienten für eine wesentliche Erweiterung und Vertiefung unseres analytischen Vermögens betrachten.*"

<sup>154</sup> Czekanowski stated: "*Die slawischen Gebiete bevölkert ein subbrachykephaler, kleinwüchsiger, dunkelblonder präslawischer Typus (β)*" (1911b: 195). The Nordic type was to be found along the Weichsel River.

<sup>155</sup> Czekanowski 1939: 86 ff.

<sup>156</sup> For example the well-known German anthropologist Hans F. K. Günther was cited in Czekanowski's 1939 article for the *Anthropologischer Anzeiger*. According to Czekanowski, Günther used this model as well and, what is more important, he incorporated Czekanowski's invention of the "Pre-Slavic type" into his works. "*Jetzt spricht H.F.K. Günther noch von der sudetischen Rasse und verweist auf unseren sublapponoiden, präslawischen Typus*" (Czekanowski 1939: 83). Günther, it should be mentioned, was among the scientists who were strongly supported by the Nazis – at least in the 1930s.

<sup>157</sup> Cf. Czekanowski 1939: 87.



Poland as well as in Germany and the Scandinavian countries.<sup>158</sup> Czekanowski's article celebrated the Polish efforts in physical anthropology and showed in detail how Polish science had influenced the international debate.<sup>159</sup>

### 3. Czekanowski's membership of various scientific organizations

Probably due to his training in Switzerland and Germany, Czekanowski was integrated into the German-speaking scientific community for a long time. He was a member of the *Gesellschaft für Physische Anthropologie* (Society of Physical Anthropology), founded in 1925. This society belonged to the *Deutsche Gesellschaft für Anthropologie, Ethnologie und Urgeschichte* and was reconstituted as the *Deutsche Gesellschaft für Rassenforschung* (German Society for Racial Research) in September 1937. Its journal, *Verhandlungen der Gesellschaft für Physische Anthropologie* (later: *Verhandlungen der Deutschen Gesellschaft für Rassenforschung*), was regularly published as a special issue of the *Anthropologischer Anzeiger* until 1940. In the last number of this periodical Czekanowski is still listed as a member of the society. This is remarkable, since the board was very interested in close contacts with the NSDAP and in promoting co-operation between the government's racial policy and science. From 1932 onwards, the secretary of the *Deutsche Gesellschaft für Rassenforschung* was Bruno Kurt Schultz, who at the same time worked as an expert in anthropology in the Office for Racial Affairs of the SS. In 1938 the *Rassenpolitische Amt* (Office of Racial Policy) and the *Deutsche Gesellschaft für Rassenforschung* signed a collaboration agreement. In the 1930s a large number of guests of honor and representatives of the Nazi Party, the state and the military attended every conference. But Czekanowski himself took part in the conferences only twice, in 1929 and in 1934.

Czekanowski was also in contact with Egon Freiherr von Eickstedt, Professor of Physical Anthropology and Ethnology in Wrocław and a member of the *Deutsche Gesellschaft für Rassenforschung* as well. But there were some disagreements between

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<sup>158</sup> Czekanowski 1939: 92. Here he wrote that "*die pessimistische These der deutschen Anthropologie von der Entnordung der europäischen Bevölkerung keine Bestätigung in den Ergebnissen der polnischen anthropologischen Forschung gefunden hat. [...] Für die Gebiete im östlichen Polen kann man dabei nachweisen, daß im Laufe des letzten Jahrtausends der Anteil der nordischen Bevölkerung stark zugenommen hat.*"

<sup>159</sup> He wrote, for example: "*Der Nachweis mendelistischer Vererbung des anthropologischen Typus bildete das vierte wichtige Ergebnis der polnischen anthropologischen Forschung*" (Czekanowski 1939: 85).

Eickstedt and his colleagues, so that they considered expelling Eickstedt from the society.<sup>160</sup> Eickstedt edited the *Zeitschrift für Rassenkunde* (Journal of Racial Anthropology) which appeared from 1935 onwards and whose international co-editors included Czekanowski until 1940.

#### 4. Czekanowski and Upper Silesia

Czekanowski's views on anthropology and in particular on the Nordic question – Nordic Poland having no reason to fear denordicization – had posed a challenge to German anthropology and to the Nazi bureaucracy. From the beginning of the Nazi regime in Germany Czekanowski's theories had been watched with suspicion.<sup>161</sup>

In September 1934 the Prussian Ministry of the Interior asked the Prussian Secret State Archives (*Preussisches Geheimes Staatsarchiv*) in Berlin-Dahlem, "whether the sections of the book *Der Mensch in Zeit und Raum* ("Man in Time and Space") which relate to Germany will be translated by the PuSt<sup>162</sup>. In the following month Walter Gross of the Office of Racial Policy asked the chairman of the *Deutsche Gesellschaft für Rassenforschung*, Otto Reche, Professor of Physical Anthropology and Ethnology in Leipzig, to give his opinion on Czekanowski's book.<sup>163</sup> Reche was not a *personal* opponent of Czekanowski. Only half a year earlier he had thanked his Polish colleague for an invitation to the second Congress of the Slavs, which he had, however, declined, adding that he would have liked to visit Warsaw again and to have met Czekanowski.<sup>164</sup> Reche himself had used formulas, mathematical graphics and tables of numbers to illustrate his anthropological studies in the past, although later he described such methods as "a time-consuming game with numbers", which "feigns results with apparent 'mathematical exactitude'" (Reche 1928: 187), so that also Czekanowski's papers needed to be "taken with a pinch of salt" (Reche 1942: 78). Reche evinced more respect for his Polish colleague than, for instance, for Eickstedt at this time.

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<sup>160</sup> For example, Eickstedt opposed the new term *Deutsche Gesellschaft für Rassenforschung* instead of *Deutsche Gesellschaft für physische Anthropologie*. He made his point in his book *Rassenkunde und Rassengeschichte der Menschheit* in 1940.

<sup>161</sup> One of the German members of the 1934 Anthropological and Ethnological sciences meeting in London complained that Czekanowski handled the race question the wrong way. Cf. Anonym 1935: "Der Pole Prof. Dr. Jan Czekanowski, Lemberg, hielt es auch für nötig, sich an der deutschen Rassenforschung zu reiben. Nach seiner Meinung herrscht das nordische Element bei den Slawen weit stärker vor als bei den Germanen ..."

<sup>162</sup> 18.9.1934; BArch R153/333.

<sup>163</sup> Cf. Office for Racial Policy to Reche on 18.10.1934; IEUL, Re IX.2.

<sup>164</sup> Cf. Reche to Czekanowski on 6.3.1934; IEUL, Re IX.5

The PuSte wrote on 5<sup>th</sup> November 1934 about *Der Mensch in Zeit und Raum*:

The contents of the book are indicated by the first few sentences of the preface, where it says: "The purpose of this book is the objective and clear representation of the latest developments of research concerning the spread of the human races. This purpose has become quite relevant because of the fact that race has been made the basic idea of the bulk of the population, who relinquish light-heartedly civil rights which they had attained in the past century."<sup>165</sup>

Czekanowski accused the German anthropologists of basing their studies on wrong assumptions. Besides, he denied that the German population was predominantly Nordic. In addition to this Czekanowski was preoccupied with the very delicate subject of the anthropology of Silesia. Parts of Upper Silesia, which had belonged to Germany until 1918, had been incorporated into Polish territory. Many people in Germany, not least the Nazis, claimed that Germany was the rightful owner of this land. In 1933 the Polish government founded in Kattowitz, Silesia, a research institution to refute the German claims. Science

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<sup>165</sup> "Der Inhalt des Buches wird durch die ersten Sätze des Vorworts gekennzeichnet, in denen es heisst: „Die Aufgabe dieses Buches ist die objektive und klare Darstellung des jetzigen Standes unserer Kenntnisse über die Verteilung der Menschenrassen. Diese wurde sehr aktuell durch den Umstand, dass die Rasse als Grundvorstellung der breiten Massen in den Vordergrund gestellt wurde, die leichten Herzens auf bürgerliche Rechte verzichten, die sie im verflossenen Jahrhundert erlangt haben“ (BArch R153/333).

"Bei ihrer tendenziösen Einstellung halten die deutschen Rassenforscher, wie z.B. zuletzt O. Reche, das nordische Gebiet Polens für eine Folgeerscheinung germanischer Einflüsse. Sie setzten die nordische Rasse mit den Germanen gleich, ohne überhaupt mit der Tatsache zu rechnen, dass fast alle Ausdehnungsbewegungen indogermanischer Völker nordisches Gepräge besaßen, mit Ausnahme der arischen Zweige, vielleicht auch der mehr mediterranen als nordischen Westgermanen. (p. 8; original p. 136) [The German race scientists, most recently O. Reche, hold the tendentious view that the Nordic region of Poland is a consequence of Teutonic influence. They have equated the Nordic race with the Teutons without even considering the fact that almost every Indo-Germanic people that expanded was Nordic except for the Aryan branches and maybe also except for the more Mediterranean than Nordic West-Teutonic ones.]

"Im Lichte unserer Ergebnisse wird offenbar, dass Deutschland keineswegs ein Land mit einem derartigen Uebergewicht der nordischen Rasse ist, wie man das allgemein glaubt. Was den rassischen Aufbau seiner Bevölkerung anbetrifft, stellt Deutschland, wie wir sahen, eine Fortsetzung Polens dar. Wenn wir dies alles berücksichtigen, so ist es schwer anzunehmen, dass der Hundertsatz des nordischen Bestandteils in Deutschland als Gesamtheit stärker als in Polen wäre. Die Rassenforschung bereitete durch die Anwendung genauerer Forschungsmethoden den Theorien der deutschen Nationalisten, die sich bemühten, mit Rassenmomenten ihre Ansicht vom erwählten Volk zu begründen, eine sehr unangenehme Ueberraschung." [In the light of our findings it becomes evident that Germany is definitely not a country with as much of a predominance of the Nordic race as is widely assumed. As far as the racial structure of the German population is concerned, it can be seen that Germany is a continuation of Poland. If we take all this into consideration, it is difficult to assume that the Nordic percentage in Germany as a whole is higher than in Poland. The application of more thorough scientific methods in race research has raised some difficulties for the German nationalists who tried to base their theory of the Chosen People on the concept of race.] (p. 14; original p., 144. BArch R153/333).

should help to secure Polish rights over Silesia. Radio programmes and public lectures should help to draw attention to the Silesian question in Poland and abroad. Polish scientists gave lectures, among them anthropologists such as Kazimierz Stolyhwo and Jan Czekanowski.<sup>166</sup> Another task of the Institute was to keep track of German efforts in the fields of Silesian anthropology, history, linguistics etc. Abstracts were made of the works of German anthropologists – especially those doing fieldwork in Silesia.<sup>167</sup>

As the German administration kept a keen interest in the work of this Silesian Institute, the papers and communiqués were translated into German. In this way the German administration was kept informed about all activities and had knowledge of what the Polish side thought about German propaganda and scientific efforts.

This was the context for Czekanowski's lecture in Kattowitz in October 1936, entitled "The Racial Structure of Silesia". A German spy immediately informed the Nazi authorities in Berlin after Czekanowski had given his lecture: "It was very successful. There were at least 300 persons assembled in the conference room of the Kattowitz hall of culture."<sup>168</sup> This might have been the reason why even the Propaganda Ministry in Berlin became interested in Czekanowski's work and asked for a copy of his lecture.

In his lecture Czekanowski pointed out again in what respect the German anthropologists were wrong. He stated:

The results of our research, based on anthropological records of the Polish army, have shown that the frontiers of Europe's anthropological territories do not correspond either to the political or to the tribal frontiers and provide irrefutable evidence that the territories of the German Empire never contained such an overwhelming Nordic element that most of the German scientists claim.<sup>169</sup>

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<sup>166</sup> The Institute published Stolyhwo's paper ("*Das Problem der rassischen Zusammensetzung der Bevölkerung Schlesiens*") as Nr. 4, 1935 in its series "Polski slask" and Czekanowski's ("*Die rassische Struktur Schlesiens nach der polnischen und der deutschen Forschung*") as Nr. 25, 1936. By 1938 a total of 36 scientific articles on Silesia had been published.

<sup>167</sup> See Communiqué des Schlesischen Instituts Nr. 31: "*Die ersten deutschen anthropologischen Untersuchungen in Oberschlesien*". Kattowitz (Berlin) 1935. In this paper Rudolf Grau, who was a student of Otto Reche, is mentioned, as well as the work of the Wrocław School.

<sup>168</sup> BArch R153/1881. "Zusammenfassung von Czekanowskis Vortrag über die Rassenstruktur Schlesiens." "*Der Erfolg war sehr gut, mindestens 300 Personen waren im Vortragssaal des Kattowitzer Bildungshauses am 23. 10. 1936 versammelt.*"

<sup>169</sup> BArch R153/1881 "*Die Ergebnisse unserer Forschungen, hinsichtlich Polens auf die heeresanthropologischen Aufnahmen gestützt, bewiesen jedoch, dass die Grenzen der anthropologischen Territorien in Europa weder mit den politischen Grenzen noch mit den Stammesgrenzen eine so klare Uebereinstimmung aufweisen, und geben uns darüber hinaus unwiderlegbarste Beweise dafür, dass das Territorium des deutschen Reiches keine Zone eines so ausgesprochenen Uebergewichts des nordischen Elementes darstellt, wie dies die Mehrheit der deutschen Forscher behauptet.*"

In particular the Wrocław School and its head, Egon Freiherr von Eickstedt, were under fire from Czekanowski on account of their contradictory findings<sup>170</sup> and inconsistency<sup>171</sup>. In the course of his lecture Czekanowski discussed and rejected the German claim that Germans were a Nordic people – arguing that the Poles were "more longheaded" than Germans and even had "fairer hair and skin and lighter eyes". His conclusion that "Poles are more Nordic than the Germans"<sup>172</sup> may well have been the reason why German anthropologists and the authorities opposed Czekanowski's ideas.

The Germans perceived Czekanowski's theses as an attack, and Karl Szodrok, NODFG-agent in Silesia, wrote immediately to the NODFG:

In the past few years in Germany publication of studies regarding race has been deferred upon orders from the authorities and the Party. One of the reasons given was that it would be unwise to reinforce the Polish-German differences concerning the *Volkstumskampf*. But since the Poles are going over to an attack, we must ask whether further silence from the German side is reasonable and whether in future we are to be content with publications in specialist journals only. In my opinion, we now need more popular representations of the Silesian racial situation.<sup>173</sup>

Szodrok suggested publishing the results of Eickstedt's anthropological survey in Upper Silesia as a work opposed to that of Czekanowski.<sup>174</sup> Reche, who was also informed about Czekanowski's lecture, wrote to Szodrok:

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<sup>170</sup> Czekanowski 1936: 3. "*Das Festhalten [v. Eickstedts] an einem so grossen Hundertsatz des nordischen Elementes bei einer so ausgesprochen kurzköpfigen Bevölkerung erfordert doch die Feststellung, dass die charakteristischste Eigentümlichkeit der nordischen Rasse der Langschädel darstelle, eine völlig unbegründete, literarische Phantasie sei.*"

<sup>171</sup> Czekanowski 1936: 10. "*Als einen völlig phantastischen Gedanken muss man die einstige Behauptung E. v. Eickstedts bezeichnen, dass der südöstliche Ausläufer Schlesiens einen Teil des ‚dinarischen‘ Gebietes darstelle. [...] Auf die These des ‚Dinarismus‘ der südöstlichen Ausläufer Schlesiens haben ihre Schöpfer sehr rasch Verzicht geleistet, wie das die Karte von I. Schwidetzky (1935, S. 196) zeigt.*"

<sup>172</sup> BArch R153/1881. Only Hans F. K. Günther was taken seriously as an anthropologist, because his work yielded results similar to Czekanowski's. Czekanowski accused Ilse Schwidetzky, Eickstedt's assistant, of "carelessness and unscrupulousness".

<sup>173</sup> Szodrok to NODFG on 28.10.1936; BArch, R153/1305. "*In den letzten Jahren wurden deutscherseits, den Weisungen der Behörden und der Partei folgend, im allgemeinen rassenkundliche Veröffentlichungen zurückgestellt, u.a. mit der Begründung, man wolle die polnisch-deutschen Gegensätze im Volkstumskampf nicht vermehren. Nachdem man aber auch hier von polnischer Seite zum Angriff übergeht, müssen wir fragen, ob ein weiteres Schweigen von deutscher Seite am Platze ist und ob man sich auch in Zukunft nur mit Veröffentlichungen in der fachwissenschaftlichen Presse begnügen will. Ich meinerseits bin der Meinung, dass wir in Schlesien, um der polnischen Propaganda begegnen zu können, von nun an auch volkstümlichere Darstellungen über die schlesischen rassenkundlichen Verhältnisse brauchen.*"

<sup>174</sup> Szodrok to NODFG on 28.10.1936; BArch, R153/1305.

Actually the lectures of Czekanowski [...] are politically not the end of the world, because in Poland he has an opponent even in the anthropologist Stolyhwo from Warsaw, who has claimed almost the opposite in just as many lectures. Cz. is a supporter of the so-called Nordic idea, and now he sees people of Nordic race everywhere in the Slav world, even in the East Baltic provinces. St. is more objective; he sees East Baltic and *ostische* [eastern] race where it really exists, and now he wants to build up and make political use of an East Baltic Idea for the Slav world – in deliberate contrast to the Nordic Idea. Both have in common that they are anti-German, and that they are trying to exploit on the one hand the Nordic and on the other hand the East Baltic Idea to justify the annexation of *Ostelbien* the country east of the Elbe River.<sup>175</sup>

In order to counteract Czekanowski's and Stolyhwo's alleged efforts to "justify the annexation of *Ostelbien*", Reche suggested that German anthropologists demonstrate that they were scientifically untenable. In his opinion Eickstedt was unfit for this job. He would just give the Polish scientist the opportunity to argue that German research was unscientific, inexact and political motivated. Since Reche found few experienced scientists to have the time to engage in an extensive conflict with the Poles, he requested an interview with the Office of Racial Policy.<sup>176</sup>

The Germans evidently had great respect for Czekanowski. Sczodrok pleaded for a representation of folk character, while Reche preferred a careful scientific work. Besides Sczodrok and Reche the *Bund Deutscher Osten* (Alliance of the German East) took part in this discussion, as well as the board of the NODFG and Werner Essen, an official from the Ministry of the Interior and NODFG agent in Lithuania.<sup>177</sup> In contrast to the others Essen took the view that continued restraint would be the best policy. Czekanowski's hypothesis of Nordic elements in Poland was less alarming than uncertain German results. He and B. K. Schultz were against racial research in the ethnically mixed border areas and in the Sorbs' provinces.<sup>178</sup>

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<sup>175</sup> Reche to Sczodrok on 7.11.1936; IEUL, Re X.2. "*An sich sind die Vorträge des Herrn Cz. [...] politisch nicht so sehr tragisch zu nehmen, da er in Polen selbst in dem Warschauer Anthropologen Stolyhwo einen Gegner hat, der beinahe das Gegenteil in ebenso zahlreichen Vorträgen sagt. Cz. ist Anhänger des sogenannten Nordischen Gedankens und sieht nun überall im Slawentum, auch in den ausgesprochen ostbaltischen Gebieten, Menschen Nordischer Rasse; St. dagegen ist objektiver, sieht ostbaltische und ostische Rasse dort, wo sie vorhanden ist, und will nun – in bewusstem Gegensatz zum Nordischen Gedanken – für das Slawentum einen ‚Ostbaltischen Gedanken‘ schaffen und politisch auswerten. Gemeinsam ist beiden allerdings, dass sie deutschfeindlich eingestellt sind und einerseits den Nordischen, andererseits den Ostbaltischen Gedanken zur Begründung von Einverleibungsansprüchen Ostelbiens auszuschlachten suchen.*"

<sup>176</sup> Reche to Sczodrok, 7.11.1936; IEUL, Re X.2.

<sup>177</sup> Cf. Sczodrok to NODFG, 9.11.1936, BArch, R153/1305.

<sup>178</sup> Cf. conversation notes, 24.11.1936; BArch, R153/1305.

Meanwhile the PuSte had translated a notice in the Polish press again. On 23 November 1936 it said:

The Germans are upset about Prof. Czekanowski. The German National Socialist press is incensed about the scientific theses of the famous Polish anthropologist, Prof. Czekanowski, on the racial structure of the population in Silesia. The press demands that German science refute the claims of Czekanowski.<sup>179</sup>

The political debate about the racial analysis of the population which began as a dispute between the German and Polish scientists, turned into a domestic conflict of anthropologists within Germany. In January 1937 Sczodrok complained about the lack of a common denominator.<sup>180</sup>

The Ministry of the Interior of the *Reich* and Prussia regarded Czekanowski's findings as quite harmless, because they were self-contradictory. Some were even considered useful as German propaganda.<sup>181</sup> In December 1937 the NSDAP *Gauleitung* in Silesia recommended to the Prussian Ministry of Science, Education and the Environment not to claim that most of the people in Silesia were *ostisch* or *ostbaltisch*. Such hypotheses would not be in accordance with scientific knowledge and might instil an inferiority complex among the people who lived near the border.<sup>182</sup> The NODFG received the same letter in March 1938.

In August 1939 Czekanowski was allowed to publish an article entitled "*Die anthropologische Struktur von Europa im Lichte polnischer Untersuchungsergebnisse*" in the *Anthropologischer Anzeiger*, the official organ of the *Deutsche Gesellschaft für Rassenforschung*. Only a few months later the Germans invaded Poland. The outbreak of war changed the situation: Eickstedt and his assistants were allowed to release their material on Silesia,<sup>183</sup> whereas Czekanowski could not publish until the end of the War.<sup>184</sup>

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<sup>179</sup> Excerpts from the Polish press (Nr. 435), Preuss. Geheimes Staatsarchiv, PuSte, on 25.11.1936; BArch, R153/1305. "*Die Deutschen regen sich auf über Prof. Czekanowski. Die deutsche nationalsozialistische Presse ist empört über die wissenschaftlichen Thesen des bekannten polnischen Anthropologen, Prof. Czekanowski, über die rassische Struktur der Bevölkerung in Schlesien. Diese Presse fordert, dass die deutsche Wissenschaft die ‚Behauptung‘ Czekanowskis zurückweist.*"

<sup>180</sup> Cf. Sczodrok to Pappritz. 30.1.1937, BArch, R153/1305.

<sup>181</sup> To NODFG, 4.3.1937; BArch R 153/1305. Czekanowski's findings were "*politische unbedenklich und z.T. sogar propagandistisch brauchbar [...], der – wenn auch etwas übertrieben – den starken nordischen Einschlag der Bevölkerung Polens betont. Wenn es politisch-propagandistisch nicht ganz einfach ist, etwa einem West- oder Süddeutschen oder gar einen Ausländer klar zu machen, dass Osten und besonders in Oberschlesien die slawischen Haussprachen [...] kein Zeichen polnischer Gesinnung und polnischen Volkstums sind, so wird diese Aufgabe ganz unmöglich gemacht, wenn in den Gebieten mit solcher Haussprache noch ein Überwiegen ‚osteuropider‘ Rasse bzw. ein Zurücktreten des nordischen Rassenanteils aufgezeigt wird.*"

<sup>182</sup> BArch R 153/1305.

In this article we have shown that Czekanowski and his papers were taken seriously by his German colleagues. We have also described the discussions that took place among academics and politicians in Poland and Germany in the interwar period, showing that in both states special interests were connected with the alleged racial stock of their own and of the neighbouring country. Convinced that clearly definable human races existed, people tried to justify territorial claims on this basis. In Germany and in Poland physical anthropologists and officials sceptically observed research on the other side of the border. Once again we can see how useful the arbitrary concept of 'race' is for political and ideological interests. Undoubtedly the concept attained a greater significance – with far more terrible consequences - in Germany, where it was widely popularised with the help of philologically orientated researchers such as H. F. K. Günther. But in both countries formulas, tables and diagrams relating to race were respected – even if some anthropologists called such approaches "a game with numbers"– because they appeared to be scientific and were hard to refute.

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<sup>183</sup> This material was published as a series called "*Rasse Volk Erbgut in Schlesien*", Breslau 1939–1941. The editors included the anthropologists von Eickstedt and Schwidetzky, but also National Socialist officials, for example "Dr. Fritz Arlt, Leiter des Rassenpolitischen Amtes bei der Gauleitung und Leiter des Landesamtes für Rassen-, Sippen- und Bevölkerungswesen bei der Provinz Schlesien".

<sup>184</sup> Further research is necessary on what happened to Czekanowski during the War. As late as 1962 he insisted on the existence of an important Nordic element in Poland and elsewhere in Europe, classifying it as "the oldest strata of the central European population" (Czekanowski 1962: 25). Together with the *paläoeuropid* (*palaäoasiatic*, *kromanid*) strata the Nordic element was the oldest element in European populations. By this time, however, his views had changed somewhat concerning the four main elements he found among the European peoples: as a fifth element he added the *Kromanid*.



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